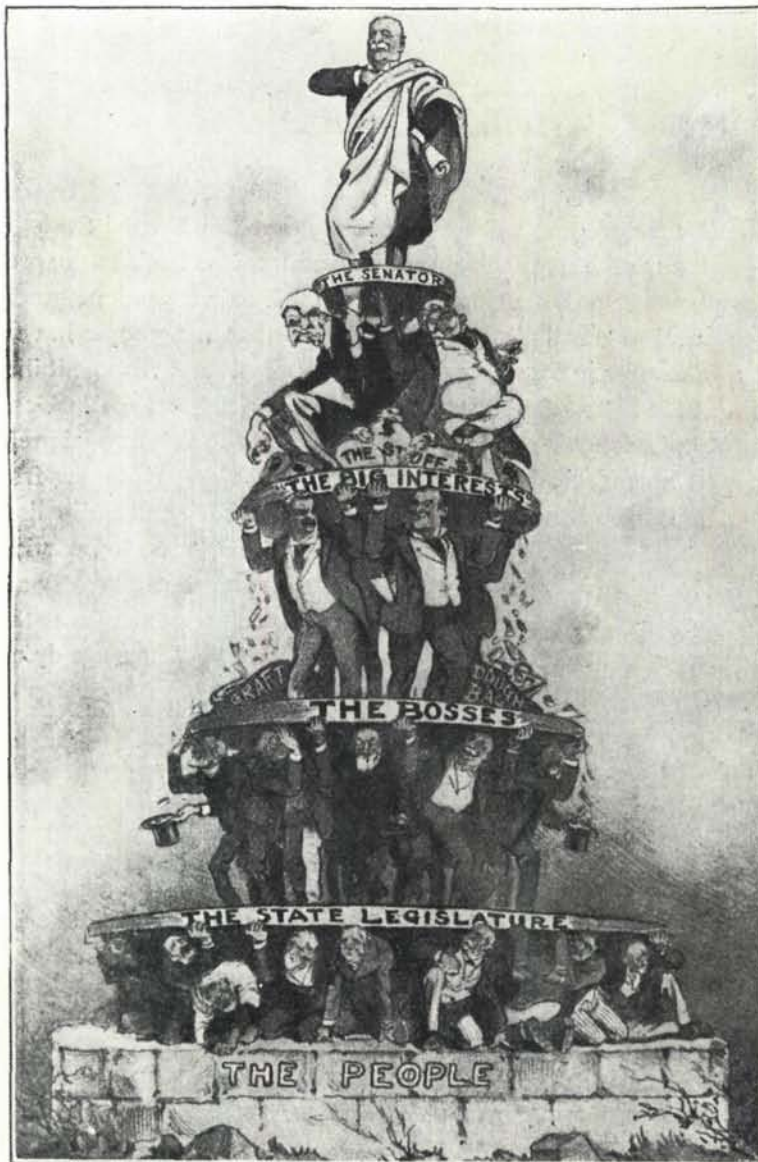


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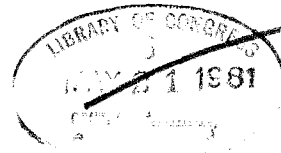
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The Discovery that Business Corrupts Politics: A Reappraisal of the Origins of Progressivism

RICHARD L. MCCORMICK

ALMOST ANY HISTORY TEXTBOOK that covers the Progressive era and was written at least twenty years ago tells how early-twentieth-century Americans discovered how big business interests were corrupting politics in quest of special privileges and how an outraged people acted to reform the perceived evils. Commonly, the narrative offers ample anecdotal evidence to support this tale of scandal and reform. The autobiographies of leading progressives—including Theodore Roosevelt, Robert M. La Follette, William Allen White, Frederic C. Howe, and Lincoln Steffens, among others—are frequently cited, because all of them recounted the purported awakening of their authors to the corrupt politico-business alliance.¹ Muckraking journalism, not only by Steffens but also by David Graham Phillips, Charles E. Russell, Ray Stannard Baker, and numerous others, is often drawn upon too, along with evidence that the magazines for which they wrote achieved unprecedented circulation. Political speeches, party platforms, and newspaper editorials by the hundreds are also offered to buttress the contention that Americans of the early 1900s discovered the prevalence of illicit business influence in politics and demanded its removal. But all of this evidence would probably fail to persuade historians today that the old textbook scenario for progressivism is correct.

And for good reason. Every prominent interpretation of the Progressive movement now encourages us not to take the outcry against politico-business corruption too seriously. Some historians have seen progressivism as dichotomous: alongside the individualist, antibusiness strain of reform stood an equally

An earlier version of this paper was read at the Seventy-Second Annual Meeting of the Organization of American Historians, held in New Orleans, April 1979. Arthur S. Link, Peyton McCrary, and David P. Thelen provided exceptionally helpful comments and suggestions on that occasion, and, in addition, I am grateful to several of my colleagues at Rutgers University for reading and commenting upon one or more drafts of the paper: Rudolph M. Bell, Paul G. E. Clemens, William L. O'Neill, Herbert H. Rowen, and Barbara M. Tucker.

¹ Although it is a common autobiographical convention to recount one's growth from ignorance to knowledge, it is nonetheless striking that so many progressive autobiographies should identify the same point of ignorance and trace a similar path to knowledge. See Roosevelt, *An Autobiography* (New York, 1913), 85–86, 186, 297–300, 306, 321–23; La Follette, *La Follette's Autobiography: A Personal Narrative of Political Experiences* (Madison, Wisc., 1960), 3–97; White, *The Autobiography of William Allen White* (New York, 1946), 149–50, 160–61, 177–79, 192–93, 215–16, 232–34, 325–26, 345, 351, 364, 428–29, 439–40, 465; Howe, *The Confessions of a Reformer* (New York, 1925), 70–72, 100–12; and Steffens, *The Autobiography of Lincoln Steffens* (New York, 1931), 357–627.

vocal, and ultimately more successful, school that accepted industrial growth and sought even closer cooperation between business and government.² Other recent interpreters have described progressivism as a pluralistic movement of diverse groups, including businessmen, who came together when their interests coincided and worked separately when they did not.³ Still other historians have seen businessmen themselves as the key progressives, whose methods and techniques were copied by other reformers.⁴ Whichever view of the movement they have favored, historians have increasingly recognized the Progressive era as the age when Americans accommodated, rather than tried to escape, large-scale business organizations and their methods.⁵ More often than not, the achievement of what used to be called reform now appears to have benefited big business interests. If our aim is to grasp the results and meaning of progressivism, the evidence in the typical textbook seems to lead in the wrong direction.

The currently dominant "organizational" interpretation of the Progressive movement has particularly little room for such evidence. Led by Samuel P. Hays and Robert H. Wiebe, a number of scholars have located the progressive impulse in the drive of newly formed business and professional groups to achieve their goals through organization and expertise. In a related study, Louis Galambos has described the progressive outcry against the trusts as merely a phase in the nation's growing acceptance of large corporations, and, with Hays and Wiebe, he has suggested that the rhetorical attack on business came to very little. The distinctive achievement of this interpretation lies in its account of how in the early twentieth century the United States became an organized, bureaucratic society whose model institution was the large corporation. Where reformers of the 1880s and 1890s had sought to resist the forces of industrialism, or at least to prevent their penetration of the local community, the progressives of the early 1900s accepted an industrial society and concentrated their efforts on controlling, ordering, and improving it. No interpretation of the era based on ideological evidence of a battle between the "people" and the "interests" can capture the enormous complexity of the adjustments to industrialism worked out by different social groups. Hays and Wiebe have succeeded better than any previous historians in describing and characterizing those adjustments and placing them in the context of large social and economic changes. In this light the

² Richard Hofstadter, *The Age of Reform: From Bryan to F.D.R.* (New York, 1955), 133; George E. Mowry, *The Era of Theodore Roosevelt, 1900-1912* (New York, 1958), 55-58; John Braeman, "Seven Progressives," *Business History Review*, 35 (1961): 581-92; and Sheldon Hackney, *Populism to Progressivism in Alabama* (Princeton, 1969), xii-xiii, 329-30.

³ John D. Buenker, "The Progressive Era: A Search for a Synthesis," *Mid-America*, 51 (1969): 175-93; David P. Thelen, "Social Tensions and the Origins of Progressivism," *Journal of American History* [hereafter, *JAH*], 56 (1969): 323-41; and Peter G. Filene, "An Obituary for 'The Progressive Movement,'" *American Quarterly*, 22 (1970): 20-34.

⁴ Robert H. Wiebe, *Businessmen and Reform: A Study of the Progressive Movement* (Cambridge, Mass., 1962); Gabriel Kolko, *The Triumph of Conservatism: A Reinterpretation of American History, 1900-1916* (New York, 1963); and Samuel P. Hays, "The Politics of Reform in Municipal Government in the Progressive Era," *Pacific Northwest Quarterly*, 55 (1964): 157-69.

⁵ Samuel P. Hays, *The Response to Industrialism, 1885-1914* (Chicago, 1957); Robert H. Wiebe, *The Search for Order, 1877-1920* (New York, 1967); Louis Galambos, *The Public Image of Big Business in America, 1880-1940: A Quantitative Study in Social Change* (Baltimore, 1975); William L. O'Neill, *The Progressive Years: America Comes of Age* (New York, 1975); and David P. Thelen, *Robert M. La Follette and the Insurgent Spirit* (Boston, 1976).

progressives' claims to have discovered and opposed the corruption of politics by business seem to become a curiosity of the era, not a clue to its meaning, a diversion to the serious historian exploring the organizational achievements that constituted true progressivism, a suitable subject for old textbooks.⁶

Despite its great strengths, however, the organizational model neglects too much.⁷ Missing is the progressives' moral intensity. Missing, too, are their surprise and animation upon discovering political and social evils. Also absent are their own explanations of what they felt and what they were doing. And absent, above all, is a description, much less an analysis, of the particular political circumstances from which progressivism emerged in the first years of the twentieth century. In place of these vivid actualities, the organizational historians offer a vague account of what motivated the reformers who advocated bureaucratic solutions and an exaggerated estimation of their capacity to predict and control events. Actually, progressive reform was not characterized by remarkable rationality or foresight; nor were the "organizers" always at the forefront of the movement. Often the results the progressives achieved were unexpected and ironical; and, along the way, crucial roles were sometimes played by men and ideas that, in the end, met defeat.

The perception that privileged businesses corrupted politics was one such ultimately unsuccessful idea of particular short-run instrumentality. Especially in the cities and states, around the middle of the first decade of the twentieth century, the discovery of such corruption precipitated crises that led to the most significant political changes of the time. When the crises had passed, the results for political participation and public policy were roughly those that the organizational interpretation predicts, but the way these changes came about is far from adequately described by that thesis. The pages that follow here sketch an account of political change in the early twentieth century and show how the discovery of politico-business corruption played this central, transforming role—though not with quite the same results that the old textbooks describe.

⁶ Louis Galambos provided a sympathetic introduction to the work of the "organizational" school; see his "The Emerging Organizational Synthesis in Modern American History," *Business History Review*, 44 (1970): 279–90. For another effort to place the work of these historians in perspective, see Robert H. Wiebe, "The Progressive Years, 1900–1917," in William H. Cartwright and Richard L. Watson, Jr., eds., *The Reinterpretation of American History and Culture* (Washington, 1973), 425–42. In addition to the works by Wiebe, Hays, and Galambos, already cited, several other studies by Hays also rank among the most important products of the organizational school: Samuel P. Hays, *Conservation and the Gospel of Efficiency: The Progressive Conservation Movement, 1890–1920* (Cambridge, Mass., 1959), "Political Parties and the Community-Society Continuum," in William Nisbet Chambers and Walter Dean Burnham, eds., *The American Party Systems: Stages of Political Development* (New York, 1967), 152–81, and "The New Organizational Society," in Jerry Israel, ed., *Building the Organizational Society: Essays on Associational Activities in Modern America* (New York, 1972), 1–15. Although Wiebe and Hays share the same broad interpretation of the period, their works make quite distinctive contributions, and there are certain matters on which they have disagreed. Some of Wiebe's most important insights concern the complex relationships between business and reform, while Hays has demonstrated particular originality on the subjects of urban politics and political parties. Concerning the middle classes, they have differing views: Wiebe has included the middle classes among the "organizers," while Hays has emphasized their persistent individualism. Compare Wiebe, *The Search for Order, 1877–1920*, chap. 5, and Hays, *The Response to Industrialism, 1885–1914*, chap. 4.

⁷ For related comments on the organizational model's shortcomings, see William G. Anderson, "Progressivism: An Historiographical Essay," *History Teacher*, 6 (1973): 427–52; David M. Kennedy, "Overview: The Progressive Era," *Historian*, 37 (1975): 453–68; O'Neill, *The Progressive Years*, x, 45; and Morton Keller, *Affairs of State: Public Life in Late-Nineteenth-Century America* (Cambridge, Mass., 1977), 285–87.

Admittedly, to interpret progressivism on the basis of its political and governmental side is a more risky endeavor than it once was. Indeed, a major thrust of contemporary scholarship has been to subordinate the Progressive era's political achievements to the larger social and economic changes associated with what Wiebe has called "the process of America's modernization."⁸ From such a perspective, "developments in politics" become, as John C. Burnham has observed, "mere epiphenomena of more basic forces and changes."⁹ But what if political behavior fails to fit trends that the rest of society seems to be experiencing? What conclusions are to be drawn, for instance, from the observation that American political rhetoric was preoccupied with attacking corporations at precisely the moment in the early twentieth century when such businesses were becoming ascendant in economic and social life? One approach simply ignores the anomalous behavior or, at most, considers it spurious or deceptive. Another answer lies in the notions that American politics is fundamentally discontinuous with the rest of national life and that, as several political scientists have suggested, it has always retained a "premodern" character.¹⁰ A better solution, however, rests upon a close study of the ways in which apparently anachronistic political events and the ideas they inspired became essential catalysts for "modernizing" developments. Studied in this manner, politics has more to tell us about progressivism than contemporary wisdom generally admits.

SHORTLY AFTER 1900, American politics and government experienced a decisive and rather rapid transformation that affected both the patterns of popular political involvement and the nature and functions of government itself. To be sure, the changes were not revolutionary, but, considering how relatively undevelopmental the political system of the United States has been, they are of considerable historical importance. The basic features of this political transformation can be easily described, but its causes and significance are somewhat more difficult to grasp.

One important category of change involved the manner and methods of popular participation in politics. For most of the nineteenth century, high rates of partisan voting—based on complex sectional, cultural, and communal influences—formed the American people's main means of political expression and involvement. Only in exceptional circumstances did most individuals or groups rely on nonelectoral methods of influencing the government. Indeed, almost no such means existed within the normal bounds of politics. After 1900, this structure of political participation changed. Voter turnout fell, and, even among those electors who remained active, pure and simple partisanship became less

⁸ Wiebe, "The Progressive Years, 1900–1917," 429.

⁹ John D. Buenker, John C. Burnham, and Robert M. Crunden, *Progressivism* (Cambridge, Mass., 1977), 4. For some disagreements among these three authors about how central politics was to progressivism, see *ibid.*, 107–29.

¹⁰ Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, 1968), 93–139; Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York, 1970), 175–93; and J. G. A. Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, 1975), 549.

pervasive. At approximately the same time, interest-group organizations of all sorts successfully forged permanent, nonelectoral means of influencing the government and its agencies. Only recently have historians begun to explore with care what caused these changes in the patterns of political participation and to delineate the redistribution of power that they entailed.¹¹

American governance, too, went through a fundamental transition in the early 1900s. Wiebe has accurately described it as the emergence of "a government broadly and continuously involved in society's operations."¹² Both the institutions of government and the content of policy reflected the change. Where the legislature had been the dominant branch of government at every level, law-makers now saw their power curtailed by an enlarged executive and, even more, by the creation of an essentially new branch of government composed of administrative boards and agencies. Where nineteenth-century policy had generally focused on distinct groups and locales (most characteristically through the distribution of resources and privileges to enterprising individuals and corporations), the government now began to take explicit account of clashing interests and to assume the responsibility for mitigating their conflicts through regulation, administration, and planning. In 1900, government did very little in the way of recognizing and adjusting group differences. Fifteen years later, innumerable policies committed officials to that formal purpose and provided the bureaucratic structures for achieving it.¹³

Most political historians consider these changes to be the products of long-term social and economic developments. Accordingly, they have devoted much of their attention to tracing the interconnecting paths leading from industrialization, urbanization, and immigration to the political and governmental responses. Some of the general trends have been firmly documented in scholarship: the organization of functional groups whose needs the established political parties could not meet; the creation of new demands for government policies to make life bearable in crowded cities, where huge industries were located; and the determination of certain cultural and economic groups to curtail the political power of people they considered threatening. All of these developments, along with others, occurred over a period of decades—now speeded, now slowed by depression, migration, prosperity, fortune, and the talents of individual men and women.

¹¹ I have elsewhere cited many of the sources on which these generalizations are based; see my "The Party Period and Public Policy: An Exploratory Hypothesis," *JAH*, 66 (1979): 279-98. On the decline in turnout and the increase in ticket-splitting, see Walter Dean Burnham, "The Changing Shape of the American Political Universe," *American Political Science Review* [hereafter, *APSR*], 59 (1965): 7-28. On the rise of interest-group organizations, see Hays, "Political Parties and the Community-Society Continuum." For two studies that make significant contributions to an understanding of how the political changes of the early twentieth century altered the power relationships among groups, see J. Morgan Kousser, *The Shaping of Southern Politics: Suffrage Restriction and the Establishment of the One-Party South, 1880-1910* (New Haven, 1974); and Carl V. Harris, *Political Power in Birmingham, 1871-1921* (Knoxville, 1977).

¹² Wiebe, *The Search for Order, 1877-1920*, 160.

¹³ McCormick, "The Party Period and Public Policy"; Robert A. Lively, "The American System: A Review Article," *Business History Review*, 29 (1955): 81-96; James Willard Hurst, *Law and the Conditions of Freedom in the Nineteenth-Century United States* (Madison, Wisc., 1956); Theodore J. Lowi, "American Business, Public Policy, Case-Studies, and Political Theory," *World Politics*, 16 (1964): 677-715; and Wiebe, *The Search for Order, 1877-1920*, 159-95.

Yet, given the long-term forces involved, it is notable how suddenly the main elements of the new political order went into place. The first fifteen years of the twentieth century witnessed most of the changes; more precisely, the brief period from 1904 to 1908 saw a remarkably compressed political transformation. During these years the regulatory revolution peaked; new and powerful agencies of government came into being everywhere.¹⁴ At the same time, voter turnout declined, ticket-splitting increased, and organized social, economic, and reform-minded groups began to exercise power more systematically than ever before.¹⁵ An understanding of how the new polity crystalized so rapidly can be obtained by exploring, first, the latent threat to the old system represented by fears of "corruption"; then, the pressures for political change that had built up by about 1904; and, finally, the way in which the old fears abruptly took on new meaning and inspired a resolution of the crisis.

LONG BEFORE 1900—indeed, since before the Revolution—Americans had been aware that governmental promotion of private interests, which became the dominant form of nineteenth-century economic policy, carried with it risks of corruption. From the English opposition of Walpole's day, colonists in America had absorbed the theory that commercial development threatened republican government in two ways: (1) by spreading greed, extravagance, and luxury among the people; and (2) by encouraging a designing ministry to conspire with monied interests for the purpose of overwhelming the independence of the legislature. Neither theme ever entirely disappeared from American politics, although each was significantly revised as time passed. For Jeffersonians in the 1790s, as Lance Banning has demonstrated, both understandings remained substantially intact. In their belief, Alexander Hamilton's program of public aid to commercial enterprises would inevitably make an agrarian people less virtuous and would also create a phalanx of privileged interests—including bank directors, speculators, and stock-jobbers—pledged to support the administration faction that had nurtured them. Even after classical republican thought waned and the structure of government-business relations changed, these eighteenth-century fears that corruption inevitably flowed from government-assisted commercial development continued to echo in American politics.¹⁶

¹⁴ James Willard Hurst, *Law and Social Order in the United States* (Ithaca, 1977), 33, 36, and *Law and the Conditions of Freedom*, 71–108; and Grover G. Huebner, "Five Years of Railroad Regulation by the States," *Annals of the American Academy of Political and Social Science*, 32 (1908): 138–56. For a further account of these governmental changes, see pages 267–69, 271–74, below.

¹⁵ Burnham, "The Changing Shape of the American Political Universe," and *Critical Elections and the Main-springs of American Politics*, 71–90, 115; and Jerrold G. Rusk, "The Effect of the Australian Ballot Reform on Split-Ticket Voting, 1876–1908," *APSR*, 64 (1970): 1220–38. For a contemporary effort to estimate and assess split-ticket voting, see Philip Loring Allen, "Ballot Laws and Their Workings," *Political Science Quarterly*, 21 (1906): 38–58.

¹⁶ Banning, *The Jeffersonian Persuasion: Evolution of a Party Ideology* (Ithaca, 1978); J. G. A. Pocock, "Virtue and Commerce in the Eighteenth Century," *Journal of Interdisciplinary History*, 3 (1972): 119–34, and *The Machiavelian Moment*, 506–52; Gordon S. Wood, *The Creation of the American Republic, 1776–1787* (Chapel Hill, 1969), 32–33, 52, 64–65, 107–14, 400–03, 416–21; Morton Keller, "Corruption in America: Continuity and Change," in Abraham S. Eisenstadt et al., eds., *Before Watergate: Problems of Corruption in American Society* (New York, 1979), 7–19; and Edwin G. Burrows, "Albert Gallatin and the Problem of Corruption in the Federalist Era," *ibid.*, 51–67.

For much of the nineteenth century, as Fred Somkin has shown, thoughtful citizens remained ambivalent about economic abundance, because they feared its potential to corrupt them and their government. "Over and over again," Somkin stated, "Americans called attention to the danger which prosperity posed for the safety of free institutions and for the maintenance of republicanism."¹⁷ In the 1830s the Democratic Party's official ideology began to give voice to these fears. Using language similar to that of Walpole's and Hamilton's critics, Andrew Jackson decried "special privileges" from government as dangerous to liberty and demanded their abolition. Much of his wrath was directed against the Second Bank of the United States. That "monster," he said, was "a vast electioneering engine"; it has "already attempted to subject the government to its will." The Bank clearly raised the question of "whether the people of the United States are to govern . . . or whether the power and money of a great corporation are to be secretly exerted to influence their judgment and control their decisions." In a different context Jackson made the point with simple clarity: "Money," he said, "is power." Yet Jackson's anti-Bank rhetoric also carried a new understanding of politico-business corruption, different from that of the eighteenth century. For the danger that Jackson apprehended came not from a corrupt ministry, whose tool the monied interests were, but from privileged monsters, acting independently from public authorities and presenting a danger not only to the government but also to the welfare of other social and economic groups ("the farmers, mechanics, and laborers") whose interests conflicted with theirs. Jackson's remedy was to scale down governmental undertakings, on the grounds that public privileges led to both corruption and inequality.¹⁸

Despite the prestige that Jackson lent to the attack on privilege, it was not a predominant fear for Americans in the nineteenth century. So many forms of thought and avarice disguised the dangers Jackson saw. First of all, Americans were far from agreed that governmental assistance for some groups hurt the rest, as he proclaimed. Both the "commonwealth" notion of a harmonious community and its successor, the Whig-Republican concept of interlocking producer interests, suggested that economic benefits from government would be shared throughout society. Even when differences emerged over who should get what, an abundance of land and resources disguised the conflicts, while the inherent divisibility of public benefits encouraged their widespread distribution. Especially at the state and local levels, Democrats, as well as Whigs and Republicans, freely succumbed to the nearly universal desire for government aid. Not to have done so would have been as remarkable as to have withheld patronage

¹⁷ Somkin, *Unquiet Eagle: Memory and Desire in the Idea of American Freedom, 1815-1860* (Ithaca, 1967), 24.

¹⁸ [Jackson] *Annual Messages, Veto Messages, Protests, &c. of Andrew Jackson, President of the United States* (Baltimore, 1835), 162, 165, 179, 197, 244. Numerous studies document the Democratic Party's use of the accusation that privileged business was corrupting politics: Lee Benson, *The Concept of Jacksonian Democracy: New York as a Test Case* (Princeton, 1961), 52-56, 96-97, 236; William G. Shade, *Banks or No Banks: The Money Issue in Western Politics, 1832-1865* (Detroit, 1972), 56-59; Marvin Meyers, *The Jacksonian Persuasion: Politics and Belief* (Stanford, 1957), 23-24, 30, 157-58, 196, 198; and Edward K. Spann, *Ideals and Politics: New York Intellectuals and Liberal Democracy, 1820-1880* (Albany, N.Y., 1972), 60, 68-78, 105-06. President Martin Van Buren's special message to Congress proposing the subtreasury system in 1837 contained accusations against the Bank similar to those Jackson had made, except that Van Buren expressed them more in "pure," eighteenth-century republican language; James D. Richardson, ed., *A Compilation of the Messages and Papers of the Presidents, 1789-1897*, 10 vols. (Washington, 1896-99), 3: 324-46.

from deserving partisans.¹⁹ Nor, in the second place, was it evident to most nineteenth-century Americans that private interests represented a threat to the commonweal. While their eighteenth-century republican heritage warned them of the danger to free government from a designing ministry that manipulated monied interests, classical economics denied that there was a comparable danger to the public from private enterprises that were independent of the government. Indeed, the public-private distinction tended to be blurred for nineteenth-century Americans, and not until it came into focus did new threats of politico-business corruption seem as real as the old ones had in the 1700s.²⁰

As time passed, Jackson's Democratic Party proved to be a weak vehicle for the insight that privileged businesses corrupted politics and government. The party's platforms, which in the 1840s had declared a national bank "dangerous to our republican institutions," afterwards dropped such rhetoric. The party of Stephen A. Douglas, Samuel J. Tilden, and Glover Cleveland all but abandoned serious criticism of politico-business corruption. Cleveland's annual message of 1887, which he devoted wholly to the tariff issue, stands as the Gilded Age's equivalent to Jackson's Bank veto. But, unlike Jackson, Cleveland made his case entirely on economic grounds and did not suggest that the protected interests corrupted government. Nor did William Jennings Bryan pay much attention to the theme in 1896. Unlike his Populist supporters who charged that public officials had "basely surrendered . . . to corporate monopolies," the Democrat Bryan made only fleeting mention of the political influence of big corporations or the danger to liberty from privileged businesses.²¹

From outside the political mainstream, the danger was more visible. Workingmen's parties, Mugwumps, Greenbackers, Prohibitionists, and Populists all voiced their own versions of the accusation that business corrupted politics and government. The Greenbackers charged that the major parties were tools of the monopolies; the Prohibitionists believed that the liquor corporations endangered free institutions; and the Populists powerfully indicted both the Democrats and Republicans for truckling to the interests "to secure corruption funds from the millionaires." In *Progress and Poverty* (1879), Henry George asked, "Is there not growing up among us a class who have all the power . . . ? We have

¹⁹ McCormick, "The Party Period and Public Policy," 286–88. On the "commonwealth" ideal, see Oscar Handlin and Mary Flug Handlin, *Commonwealth—A Study of the Role of Government in the American Economy: Massachusetts, 1774–1861* (New York, 1947); and Louis Hartz, *Economic Policy and Democratic Thought: Pennsylvania, 1776–1860* (Cambridge, Mass., 1948). For a classic expression of the Whig concept of interlocking producer interests, see Calvin Colton, ed., *The Works of Henry Clay, Comprising His Life, Correspondence, and Speeches*, 5 (New York, 1897): 437–86; and, for a later Republican expression of the same point of view, see Benjamin Harrison, *Speeches of Benjamin Harrison, Twenty-Third President of the United States* (New York, 1892), 62, 72, 157, 167, 181, 197. For a discussion of the Republican ideology and economic policy, see Eric Foner, *Free Soil, Free Labor, Free Men: The Ideology of the Republican Party before the Civil War* (New York, 1970), 18–23.

²⁰ Lively, "The American System," 94; Carter Goodrich, "The Revulsion against Internal Improvements," *Journal of Economic History*, 10 (1950): 169; and Hays, *The Response to Industrialism, 1885–1914*, 39–40. On the reluctance of state legislatures to prohibit their members from mixing public and private business, see Ari Hoogenboom, "Did Gilded Age Scandals Bring Reform?" in Eisenstadt et al., *Before Watergate*, 127–31.

²¹ Compare the Democratic platforms of 1840–52 with those for the rest of the century; see Donald Bruce Johnson and Kirk H. Porter, eds., *National Party Platforms, 1840–1972* (Urbana, 1973); for the People's Party platform of 1896, see *ibid.*, 104. For Cleveland's message of 1887, see Richardson, *Messages and Papers of the Presidents, 1789–1897*, 8: 580–91; and, for a compilation of Bryan's speeches of 1896, see his *The First Battle: A Story of the Campaign of 1896* (Chicago, 1896).

simple citizens who control thousands of miles of railroad, millions of acres of land, the means of livelihood of great numbers of men; who name the governors of sovereign states as they name their clerks, choose senators as they choose attorneys, and whose will is as supreme with legislatures as that of a French king sitting in a bed of justice."²² But these were the voices of dissenters and frail minorities. Their accusations of corruption posed a latent challenge to an economic policy based on distributing privileges to private interests, but for most of the nineteenth century their warnings were not widely accepted or even listened to by the political majority.

The late 1860s and early and mid-1870s, however, offer an apparent exception. These were the years when the *Crédit Mobilier* and other scandals—local and national—aroused a furor against politico-business corruption. "Perhaps the offense most discredited by the exposures," according to C. Vann Woodward, "was the corrupting of politicians to secure government subsidies and grants to big corporations—particularly railroads." For several years, in consequence, there was a widespread revulsion against a policy of bestowing public privileges and benefits on private companies. Editorializing in 1873 on the *Crédit Mobilier* scandal, E. L. Godkin of the *Nation* declared, "The remedy is simple. The Government must get out of the 'protective' business and the 'subsidy' business and the 'improvement' and the 'development' business. It must let trade, and commerce, and manufactures, and steamboats, and railroads, and telegraphs alone. It cannot touch them without breeding corruption." Yet even in the mid-1870s, by Woodward's own account, it was possible for railroad and other promoters, especially in the South and Midwest, to organize local meetings that rekindled the fervor for subsidies in town after town. The fear of corruption that Godkin voiced simply was not compelling enough to override the demand for policies of unchecked promotion.²³

Even the nineteenth century's most brilliant and sustained analysis of business and politics—that provided by the Adams brothers, Charles Francis, Jr. and Henry, in their *Chapters of Erie* (1871)—failed to portray the danger convincingly. Recounting the classic Gilded Age roguery of Jay Gould and Jim Fisk, including their corruption of courts and legislatures and their influence on the president himself, the Adamses warned that, as Henry put it, "the day is at hand when corporations . . .—having created a system of quiet but irresistible corruption—will ultimately succeed in directing government itself." But the Adams brothers presented Gould and Fisk as so fantastic that readers could not believe that ordinary businessmen could accomplish such feats. Rather than de-

²² Johnson and Porter, *National Party Platforms, 1840-1972*, 90; and George, *Progress and Poverty—An Inquiry into the Cause of Industrial Depressions and of Increase of Want with Increase of Wealth: The Remedy* (New York, 1880), 481. For examples of other late-nineteenth-century dissenters who recognized the corruption of politics and government by business interests, see H. R. Chamberlain, *The Farmers' Alliance: What It Aims to Accomplish* (New York, 1891), 12, 37-38; and Henry Demarest Lloyd, *Wealth against Commonwealth* (New York, 1894), 369-404.

²³ Woodward, *Reunion and Reaction: The Compromise of 1877 and the End of Reconstruction* (Boston, 1951), 65; and Godkin, "The Moral of the *Crédit Mobilier* Scandal," *Nation*, 16 (1873): 68. Also see Allan Nevins, *The Emergence of Modern America, 1865-1878* (New York, 1927), 178-202; and John G. Sproat, "The Best Men": *Liberal Reformers in the Gilded Age* (New York, 1968), 72-73. For the ebb and flow of public aid to private enterprise in this era, see Keller, *Affairs of State*, 162-96. For other expressions of Godkin's opinion, see the *Nation*, 16 (1873): 328-29, and 24 (1877): 82-83.

scribing a process of politico-business corruption, the Adamses gave only the dramatic particulars of it. Words like “astounding,” “unique,” and “extraordinary” marked their account. Writing of the effort by Gould and Fisk to corner the market on gold in 1869, Henry said, “Even the most dramatic of modern authors, even Balzac himself, . . . or Alexandre Dumas, with all his extravagance of imagination, never have reached a conception bolder or more melodramatic than this, nor have they ever ventured to conceive a plot so enormous, or a catastrophe so original.” Far from supporting the Adamses’ thesis, such descriptions must have undermined it by raising doubts that what Gould and Fisk did could be widely or systematically repeated.²⁴

Expressed by third parties and by elite spokesmen like Godkin and the Adamses, the fear that business corrupted politics exerted only minor influence in the late nineteenth century. When they recognized corruption, ordinary people seem to have blamed “bad” politicians, like James G. Blaine, and to have considered the businessmen guiltless. Even when Americans saw that corruption involved the use of money, they showed more interest in how the money was spent—for example, to bribe voters—than in where it came from. Wanting governmental assistance for their enterprises, but only sporadically scrutinizing its political implications, most people probably failed to perceive what the Adamses saw.²⁵ Nor did they, until social and industrial developments created deep dissatisfaction with the existing policy process. Then, the discovery that privileged businesses corrupted politics played a vital, if short-lived, role in facilitating the momentous transition from the nineteenth-century polity to the one Americans fashioned at the beginning of the twentieth century.

By the 1890s, LARGE-SCALE INDUSTRIALIZATION was creating the felt need for new government policies in two distinct but related ways. The first process, which Hays and Wiebe have described so well, was the increasing organization of diverse producer groups, conscious of their own identities and special needs. Each demanded specific public protections for its own endeavors and questioned the allocation of benefits to others. The second development was less tangible: the unorganized public’s dawning sense of vulnerability, unease, and anger in the face of economic changes wrought by big corporations. Sometimes, the people’s inchoate feelings focused on the ill-understood “trusts”; at other times, their negative emotions found more specific, local targets in street-railway or electric-power companies. Older interpretations of progressivism gave too much weight to the second of these developments; recently, only a few historians have sufficiently recognized it.²⁶

²⁴ Adams and Adams, *Chapters of Erie* (reprint ed., Ithaca, 1956), 136, 107. Originally published as articles during the late 1860s and early 1870s, these essays were first issued in book form in 1871 under the title *Chapters of Erie and Other Essays* (Boston).

²⁵ For the vivid expression of a similar point, see Wiebe, *The Search for Order, 1877–1920*, 28.

²⁶ Hays, *The Response to Industrialism, 1885–1914*; and Wiebe, *The Search for Order, 1877–1920*. On the fear and anger of the unorganized, see Hofstadter, *The Age of Reform*, 213–69; Irwin Unger and Debi Unger, *The Vulnerable Years: The United States, 1896–1917* (Hinsdale, Ill., 1977), 102–08; and David P. Thelen, *The New Citizenship: Origins of Progressivism in Wisconsin, 1885–1900* (Columbia, Mo., 1972).

Together, these processes created a political crisis by making people conscious of uncomfortable truths that earlier nineteenth-century conditions had obscured: that society's diverse producer groups did not exist in harmony or share equally in government benefits, and that private interests posed a danger to the public's interests. The crisis brought on by the recognition of these two problems extended approximately from the onset of depression in 1893 until 1908 and passed through three distinct phases: (1) the years of realignment, 1893–96; (2) the years of experimentation and uncertainty, 1897–1904; and (3) the years of discovery and resolution, 1905–08. When the crisis was over, the American political system was different in important respects from what it had been before.

During the first phase, the depression and the alleged radicalism of the Populists preoccupied politics and led to a decisive change in the national balance of party power. Willingly or unwillingly, many former voters now ceased to participate in politics, while others from almost every social group in the North and Midwest shifted their allegiance to the Republicans. As a result, that party established a national majority that endured until the 1930s. Yet, given how decisive the realignment of the 1890s was, it is striking how quickly the particular issues of 1896—tariff protection and free silver—faded and how little of long-standing importance the realignment resolved.²⁷ To be sure, the defeat of Bryan and the destruction of Populism established who would not have control of the process of accommodating the nation to industrial realities, but the election of 1896 did much less in determining who would be in charge or what the solutions would be.

In the aftermath of realignment, a subtler form of crisis took hold—although several happy circumstances partially hid it, both from people then and from historians since. The war with Spain boosted national pride and self-confidence; economic prosperity returned after the depression; and the Republican Party with its new majority gave the appearance of having doctrines that were relevant to industrial problems. Soon, President Theodore Roosevelt's activism and appeal helped foster an impression of political command over the economy. However disguised, the crisis nonetheless was real, and, in the years after 1896, many voices quietly questioned whether traditional politics and government could resolve interest-group conflicts or allay the sense of vulnerability that ordinary people felt.

Central to the issue were the dual problems of how powerful government should be and whether it ought to acknowledge and adjust group differences. Industrialism and its consequences seemed to demand strong public policies based on a recognition of social conflict. At the very least, privileged corporations had to be restrained, weaker elements in the community protected, and

²⁷ The three most important studies of the electoral realignment of the 1890s are Paul Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850–1900* (New York, 1970); Richard Jensen, *The Winning of the Midwest: Social and Political Conflict, 1888–1896* (Chicago, 1971); and Samuel T. McSeveney, *The Politics of Depression: Political Behavior in the Northeast, 1893–1896* (New York, 1972). A number of studies associate the realignment with subsequent changes in government policy: Walter Dean Burnham *et al.*, "Partisan Realignment: A Systemic Perspective," in Joel H. Silbey *et al.*, eds., *The History of American Electoral Behavior* (Princeton, 1978), 45–77; and David W. Brady, "Critical Elections, Congressional Parties, and Clusters of Policy Changes," *British Journal of Political Science*, 8 (1978): 79–99.

regular means established for newer interest groups to participate in government. But the will, the energy, and the imagination to bring about these changes seemed missing. Deeply felt ideological beliefs help explain this paralysis. The historic American commitment, on the one hand, to weak government, local autonomy, and the preservation of individual liberties—reflected in the doctrines of the Democratic Party—presented a strong barrier to any significant expansion of governmental authority. The ingrained resistance, on the other hand, to having the government acknowledge that the country's producing interests were not harmonious—voiced in the doctrines of the Republican Party—presented an equally strong obstacle to the recognition and adjustment of group differences.²⁸

Weighted down by their doctrines as well as by an unwillingness to alienate elements of their heterogeneous coalitions, both parties floundered in attempting to deal with these problems. The Democrats were merely more conspicuous in failing than were the Republicans. Blatantly divided into two wings, neither of which succeeded in coming to grips with the new issues, the Democrats blazoned their perplexity by nominating Bryan for president for a second time in 1900, abandoning him for the conservative Alton B. Parker in 1904, and then returning to the Great Commoner (who was having trouble deciding whether to stand for nationalizing the railroads) in 1908. The Republicans, for their part, were only a little less contradictory in moving from McKinley to Roosevelt to Taft. Roosevelt, moreover, for all of the excitement he brought to the presidency in 1901, veered wildly in his approach to the problems of big business during his first term—from “publicity” to trust-busting to jawboning to conspiring with the House of Morgan.²⁹

While the national leaders wavered and confidence in the parties waned, a good deal of experimenting went on in the cities and states—much of it haphazard and unsuccessful. Every large city found it difficult to obtain cheap and efficient utilities, equitable taxes, and the variety of public services required by an expanding, heterogeneous population. A few, notably Detroit and later Cleveland and New York, made adjustments during the last years of the nineteenth and the first years of the twentieth centuries that other cities later copied: the adoption of restrictions on utility and transportation franchises, the imposition of new taxes on intangible personalty, and the inauguration of innovative municipal services. But most cities were less successful in aligning governance with industrialism. Utility regulation was a particularly difficult problem. Franchise “grabs” agreed to by city councilmen came under increasing attack, but the

²⁸ For a discussion of the major parties' ideological beliefs, see Robert Kelley, “Ideology and Political Culture from Jefferson to Nixon,” *AHR*, 82 (1977): 531–62. And, for a brilliant account of the resistance to change, see Keller, *Affairs of State*.

²⁹ On the Democratic Party's doctrinal floundering in these years, see J. Rogers Hollingsworth, *The Whirligig of Politics: The Democracy of Cleveland and Bryan* (New York, 1963). For the Republican side of the story, see Nathaniel W. Stephenson, *Nelson W. Aldrich: A Leader in American Politics* (New York, 1930); and John M. Blum, *The Republican Roosevelt* (Cambridge, Mass., 1954). Roosevelt's doctrinal uncertainties can be traced in his annual messages as president; see Hermann Hagedorn, ed., *The Works of Theodore Roosevelt*, memorial edition, 17 (New York, 1925): 93–641. For a recent treatment of these matters, see Lewis L. Gould, *Reform and Regulation: American Politics, 1900–1916* (New York, 1978).

chaotic competition between divergent theories of regulation (home rule versus state supervision versus municipal ownership) caused the continuance of poor public policy.³⁰ In the states, too, the late 1890s and early 1900s were years of experimentation with various methods of regulation and administration. What Gerald D. Nash has found for California seems to have been true elsewhere as well: the state's railroad commission "floundered" in the late nineteenth century due to ignorance, inexperience, and a lack of both manpower and money. These were, Nash says, times of "trial and error." Antitrust policy also illuminates the uncertainty that was characteristic of the period before about 1905. By the turn of the century, two-thirds of the states had already passed antitrust laws, but in the great majority the provisions for enforcement were negligible. Some states simply preferred encouraging business to restraining it; others felt that the laxity of neighboring states and of the federal government made antitrust action futile; still others saw their enforcement policies frustrated by court decisions and administrative weaknesses. The result was unsuccessful policy—and a consequent failure to relieve the crisis that large-scale industrialization presented to nineteenth-century politics and government.³¹

In September 1899, that failure was searchingly probed at a conference on trusts held under the auspices of the Chicago Civic Federation. Attended by a broad spectrum of the country's political figures and economic thinkers, the meeting's four days of debates and speeches amply expressed the agitation, the uncertainty, and the discouragement engendered by the nation's search for solutions to the problems caused by large business combinations. In exploring whether and to what extent the government should regulate corporations and how to adjust social-group differences, the speakers addressed basic questions about the nineteenth-century American polity.³² Following the conference, the search for answers continued unabated, for there was little consensus and considerable resistance to change. In the years immediately following, pressure to do *something* mounted. And roughly by the middle of the next decade, many of the elements were in place for a blaze of political innovation. The spark that finally served to ignite them was a series of disclosures reawakening and refashioning the old fear that privileged business corrupted politics and government.

THE EVIDENCE CONCERNING THESE DISCLOSURES is familiar to students of progressivism, but its meaning has not been fully explored. The period 1904–08 com-

³⁰ Melvin G. Holli, *Reform in Detroit: Hazen S. Pingree and Urban Politics* (New York, 1969); Martin J. Schiesl, *The Politics of Efficiency: Municipal Administration and Reform in America, 1880–1920* (Berkeley and Los Angeles, 1977); Mowry, *The Era of Theodore Roosevelt, 1900–1912*, 59–67; Thelen, *The New Citizenship*, 130–201; and David Nord, "The Experts versus the Experts: Conflicting Philosophies of Municipal Utility Regulation in the Progressive Era," *Wisconsin Magazine of History*, 58 (1975): 219–36.

³¹ Nash, "The California Railroad Commission, 1876–1911," *Southern California Quarterly*, 44 (1962): 293, 303; Harry L. Purdy et al., *Corporate Concentration and Public Policy* (2d ed., New York, 1950), 317–22; Hans B. Thorelli, *The Federal Antitrust Policy: Origination of an American Tradition* (Baltimore, 1955), 155–56, 265, 352–55, 607; and William Letwin, *Law and Economic Policy in America: The Evolution of the Sherman Antitrust Act* (New York, 1965), 182–247.

³² Civic Federation of Chicago, *Chicago Conference on Trusts* (Chicago, 1900).

prised the muckraking years, not only in national magazines but also in local newspapers and legislative halls across the country. During 1905 and 1906 in particular, a remarkable number of cities and states experienced wrenching moments of discovery that led directly to significant political changes. Usually, a scandal, an investigation, an intraparty battle, or a particularly divisive election campaign exposed an illicit alliance of politics and business and made corruption apparent to the community, affecting party rhetoric, popular expectations, electoral behavior, and government policies.³³

Just before it exploded in city and state affairs, business corruption of politics had already emerged as a leading theme of the new magazine journalism created by the muckrakers. Their primary contribution was to give a national audience the first systematic accounts of how modern American society operated. In so doing, journalists like Steffens, Baker, Russell, and Phillips created insights and pioneered ways of describing social and political relationships that crucially affected how people saw things in their home towns and states. Since so many of the muckrakers' articles identified the widespread tendency for privilege-seeking businessmen to bribe legislators, conspire with party leaders, and control nominations, an awareness of such corruption soon entered local politics. Indeed, many of the muckraking articles concerned particular locales—including Steffens's early series on the cities (1902–03); his subsequent exposures of Missouri, Illinois, Wisconsin, Rhode Island, New Jersey, and Ohio (1904–05); Rudolph Blankenburg's articles on Pennsylvania (1905); and C. P. Connolly's treatment of Montana (1906). All of these accounts featured descriptions of politico-business corruption, as did many of the contemporaneous exposures of individual industries, such as oil, railroads, and meat-packing. Almost immediately after this literature began to flourish, citizens across the country discovered local examples of the same corrupt behavior that Steffens and the others had described elsewhere.³⁴

In New York, the occasion was the 1905 legislative investigation of the life insurance industry. One by one, insurance executives and Republican politicians took the witness stand and were compelled to bare the details of their corrupt relations. The companies received legislative protection, and the Republicans got bribes and campaign funds. In California, the graft trials of San Francisco city officials, beginning in 1906, threw light on the illicit cooperation between

³³ For other analyses that indicate the importance of the year 1906 in state politics around the country, see Richard M. Abrams, *Conservatism in a Progressive Era: Massachusetts Politics, 1900–1912* (Cambridge, Mass., 1964), 131; and Dewey W. Grantham, Jr., "The Progressive Era and the Reform Tradition," *Mid-America*, 46 (1964): 233–35.

³⁴ The fullest treatment of the muckrakers is still Louis Filler's *The Muckrakers*, a new and enlarged edition of his *Crusaders for American Liberalism* (University Park, Pa., 1976). Filler's chronology provides a convenient list of the major muckraking articles; *ibid.*, 417–24. Steffens's initial series on the cities was published as *The Shame of the Cities* (New York, 1904). His subsequent articles on the states appeared in *McClure's Magazine* between April 1904 and July 1905; these essays were later published as *The Struggle for Self-Government* (New York, 1906). Blankenburg's articles on Pennsylvania appeared in *The Arena* between January and June 1905; Connolly's "The Story of Montana" was published in *McClure's Magazine* between August and December 1906. Other major magazine articles probing politico-business corruption include "The Confessions of a Commercial Senator," *World's Work*, April–May 1905; Charles Edward Russell, "The Greatest Trust in the World" [the meat-packing industry], *Everybody's Magazine*, 1905; and David Graham Phillips, "The Treason of the Senate," *Cosmopolitan Magazine*, 1906.

businessmen and public officials. Boss Abraham Ruef had delivered special privileges to public utility corporations in return for fees, of which he kept some and used the rest to bribe members of the city's Board of Supervisors. San Francisco's awakening revitalized reform elsewhere in California, and the next year insurgent Republicans formally organized to combat their party's alliance with the Southern Pacific Railroad. In Vermont, the railroad commissioners charged the 1906 legislature with yielding "supinely to the unfortunate influence of railroad representatives." Then the legislature investigated and found that the commissioners themselves were corrupt!³⁵

Other states, in all parts of the country, experienced their own versions of these events during 1905 and 1906. In South Dakota, as in a number of Midwestern states, hostility to railroad influence in politics—by means of free passes and a statewide network of paid henchmen—was the issue around which insurgent Republicans coalesced against the regular machine. Some of those who joined the opposition did so purely from expediency; but their charges of corruption excited the popular imagination, and they captured the state in 1906 with pledges of electoral reform and business regulation. Farther west Denver's major utilities, including the Denver Tramway Company and the Denver Gas and Electric Company, applied for new franchises in 1906, and these applications went before the voters at the spring elections. When the franchises all narrowly carried, opponents of the companies produced evidence that the Democratic and Republican Parties had obtained fraudulent votes for the utilities. The case made its way through the courts during the next several months, and, although they ultimately lost, Colorado's nascent progressives derived an immense boost from the well-publicized judicial battle. As a result, the focus of reform shifted to the state. Dissidents in the Republican Party organized to demand direct primary nominations and a judiciary untainted by corporate influence. These questions dominated Colorado's three-way gubernatorial election that fall.³⁶

To the south, in Alabama, Georgia, and Mississippi, similar accusations of politico-business corruption were heard that same year, only in a different regional accent. In Alabama, Braxton Bragg Comer rode the issue from his position on the state's railroad commission to the governorship. His "main theme," according to Sheldon Hackney, "was that the railroads had for years deprived the people of Alabama of their right to rule their own state and that the time had come to free the people from alien and arbitrary rule." Mississippi voters heard similar rhetoric from Governor James K. Vardaman in his unsuccessful campaign against John Sharp Williams for a seat in the U. S. Senate. Georgia's

³⁵ Robert F. Wessner, *Charles Evans Hughes: Politics and Reform in New York, 1905-1910* (Ithaca, 1967), 18-69; Richard L. McCormick, *From Realignment to Reform: Political Change in New York State, 1893-1910* (Ithaca, 1981), chap. 7; George E. Mowry, *The California Progressives* (Berkeley and Los Angeles, 1951), 23-85; Spencer C. Olin, Jr., *California's Prodigal Sons: Hiram Johnson and the Progressives, 1911-1917* (Berkeley and Los Angeles, 1968), 1-19; Winston Allen Flint, *The Progressive Movement in Vermont* (Washington, 1941), 42-51; and the *Tenth Biennial Report of the Board of Railroad Commissioners of the State of Vermont* (Bradford, Vt., 1906), 25.

³⁶ Herbert S. Schell, *History of South Dakota* (Lincoln, Neb., 1961), 258-61; Fred Greenbaum, "The Colorado Progressives in 1906," *Arizona and the West*, 7 (1965): 21-32; and Carl Abbott, *Colorado: A History of the Centennial State* (Boulder, 1976), 203-06.

Tom Watson conjured up some inane but effective imagery to illustrate how Vardaman's opponent would serve the business interests: "If the Hon. John Sharp Williams should win out in the fight with Governor Vardaman, the corporations would have just one more doodle-bug in the United States Senate. Every time that a Railroad lobbyist stopped over the hole and called 'Doodle, Doodle, Doodle'—soft and slow—the sand at the little end of the funnel would be seen to stir, and then the little head of J. Sharp would pop up." In Watson's own state, Hoke Smith trumpeted the issue, too, in 1905 and 1906.³⁷

New Hampshire, Rhode Island, New Jersey, Pennsylvania, Ohio, Indiana, North Dakota, Nebraska, Texas, and Montana, among other states, also had their muckraking moments during these same years. Although the details varied from place to place, there were three basic routes by which the issue of politico-business corruption entered state politics. In some states, including New York, Colorado, and California, a legislative investigation or judicial proceeding captured attention by uncovering a fresh scandal or by unexpectedly focusing public attention on a recognized political sore. Elsewhere, as in New Hampshire, South Dakota, and Kansas, a factional battle in the dominant Republican Party inspired dissidents to drag their opponents' misdeeds into public view; in several Southern states, the Democrats divided in similar fashion, and each side told tales of the other's corruption by business interests. Finally, city politics often became a vehicle for spreading the issue of a politico-business alliance to the state. Philadelphia, Jersey City, Cincinnati, Denver, and San Francisco all played the role of inspiring state reform movements based on this issue. Some states took more than one of these three routes; and the politicians and reformers in a few states simply echoed what their counterparts elsewhere were saying without having any outstanding local stimulus for doing so. This pattern is, of course, not perfect. In Wisconsin and Oregon, the discovery of politico-business corruption came earlier than 1905–06; in Virginia its arrival engendered almost no popular excitement, while it scarcely got to Massachusetts at all.³⁸

An anonymous Kansan, whose state became aware of business domination of

³⁷ Hackney, *Populism to Progressivism in Alabama*, 257; Watson's *Weekly Jeffersonian*, July 25, 1907, as quoted in William F. Holmes, *The White Chief: James Kimble Vardaman* (Baton Rouge, 1970), 184; Dewey W. Grantham, Jr., *Hoke Smith and the Politics of the New South* (Baton Rouge, 1958), 131–46; and C. Vann Woodward, *Origins of the New South, 1877–1913* (Baton Rouge, 1951), 369–95.

³⁸ Geoffrey Blodgett, "Winston Churchill: The Novelist as Reformer," *New England Quarterly*, 47 (1974): 495–517; Thomas Agan, "The New Hampshire Progressives: Who and What Were They?" *Historical New Hampshire*, 34 (1979): 32–53; Charles Carroll, *Rhode Island: Three Centuries of Democracy*, 2 (New York, 1932): 676–78; Erwin L. Levine, *Theodore Francis Green: The Rhode Island Years, 1906–36* (Providence, 1963), 1–19; Arthur S. Link, *Wilson: The Road to the White House* (Princeton, 1947), 133–40; Ransom E. Noble, Jr., *New Jersey Progressivism before Wilson* (Princeton, 1946), 24–81; Eugene M. Tobin, "The Progressive as Politician: Jersey City, 1896–1907," *New Jersey History*, 91 (1973): 5–23; Lloyd M. Abernethy, "Insurgency in Philadelphia, 1905," *Pennsylvania Magazine of History and Biography*, 87 (1963): 3–20; Hoyt Landon Warner, *Progressivism in Ohio, 1897–1917* (Columbus, 1964), 143–210; Clifton J. Phillips, *Indiana in Transition: The Emergence of an Industrial Commonwealth, 1880–1920* (Indianapolis, 1968), 93–100; Charles N. Glaab, "The Failure of North Dakota Progressivism," *Mid-America*, 39 (1957): 195–209; James C. Olson, *History of Nebraska* (Lincoln, Neb., 1955), 250–53; Alwyn Barr, *Reconstruction to Reform: Texas Politics, 1876–1906* (Austin, 1971), 229–42; Michael P. Malone and Richard B. Roeder, *Montana: A History of Two Centuries* (Seattle, 1976), 196–99; Robert S. Maxwell, *La Follette and the Rise of the Progressives in Wisconsin* (Madison, Wisc., 1956); Herbert F. Margulies, *The Decline of the Progressive Movement in Wisconsin, 1890–1920* (Madison, Wisc., 1968); Raymond H. Pulley, *Old Virginia Restored: An Interpretation of the Progressive Impulse, 1870–1930* (Charlottesville, 1968); and Abrams, *Conservatism in a Progressive Era*.

its politics and government in 1905 and 1906, later gave a description of the discovery that also illuminates what happened elsewhere. When he first entered politics in the 1890s, the Kansan recalled, "three great railroad systems governed" the state. "This was a matter of common knowledge, but nobody objected or was in any way outraged by it." Then "an awakening began" during Roosevelt's first term as president, due to his "hammering on the square deal" and to a growing resentment of discriminatory railroad rates. Finally, after the railroads succeeded in using their political influence to block rate reform, "it began to dawn upon me," the Kansan reported, "that the railway contributions to campaign funds were part of the general game. . . . I saw they were in politics so that they could run things as they pleased." He and his fellow citizens had "really been converted," he declared. "We have got our eyes open now. . . . We have seen that the old sort of politics was used to promote all sorts of private ends, and we have got the idea now that the new politics can be used to promote the general welfare."³⁹

State party platforms provide further evidence of the awakening to politico-business corruption. In Iowa, to take a Midwestern state, charges of corporation influence in politics were almost entirely confined to the minor parties during the years from 1900 to 1904. Prohibitionists believed that the liquor industry brought political corruption, while socialists felt that the powers of government belonged to the capitalists. For their part, the Democrats and Republicans saw little of this—until 1906, when both major parties gushed in opposition to what the Republicans now called "the domination of corporate influences in public affairs." The Democrats agreed: "We favor the complete elimination of railway and other public service corporations from the politics of the state." In Missouri, a different but parallel pattern emerges from the platforms. There, what had been a subordinate theme of the Democratic Party (and minor parties) in 1900 and 1902 became of central importance to both parties in 1904 and 1906. The Democrats now called "the eradication of bribery" the "paramount issue" in the state and declared opposition to campaign contributions "by great corporations and by those interested in special industries enjoying special privileges under the law." In New Hampshire, where nothing had been said of politico-business corruption in 1900 and 1904, both major parties wrote platforms in 1906 that attacked the issuance of free transportation passes and the prevalence of corrupt legislative lobbies. Party platforms in other states also suggest how suddenly major-party politicians discovered that business corrupted politics.⁴⁰

³⁹ "How I Was Converted—Politically: By a Kansas Progressive Republican," *Outlook*, 96 (1910): 857–59. Also see Robert Sherman La Forte, *Leaders of Reform: Progressive Republicans in Kansas, 1900–1916* (Lawrence, Kansas, 1974), 13–88.

⁴⁰ *The Iowa Official Register for the Years 1907–1908* (Des Moines, 1907), 389, 393; *Official Manual of the State of Missouri for the Years 1905–1906* (Jefferson City, Mo., 1905), 254; and *Official Manual of the State of Missouri for the Years 1907–1908* (Jefferson City, Mo., 1907), 365. Also see State of New Hampshire, *Manual for the General Court, 1907* (Concord, N.H., 1907), 61–63. State party platforms for the early 1900s are surprisingly hard to locate. For some states, particularly in the Northeast and Midwest, the platforms were printed in the annual legislative manuals and blue books, but otherwise they must be found in newspapers. Of the ten states—Iowa, Missouri, New Hampshire, New York, New Jersey, Indiana, Pennsylvania, Illinois, Wisconsin, and South Dakota—for which I was able to survey the party platforms of 1900–10 fairly completely (using the manuals,

The annual messages of the state governors from 1902 to 1908 point to the same pattern. In the first three years, the chief executives almost never mentioned the influence of business in politics. Albert Cummins of Iowa was exceptional; as early as 1902 he declared, "Corporations have, and ought to have, many privileges, but among them is not the privilege to sit in political conventions or occupy seats in legislative chambers." Then in 1905, governors across the Midwest suddenly let loose denunciations of corporate bribery, lobbying, campaign contributions, and free passes. Nebraska's John H. Mickey was typical in attacking "the onslaught of private and corporation lobbyists who seek to accomplish pernicious ends by the exercise of undue influence." Missouri's Joseph W. Folk advised that "all franchises, rights and privileges secured by bribery should be declared null and void." By 1906, 1907, and 1908, such observations and recommendations were common to the governors of every region. In 1907 alone, no less than nineteen state executives called for the regulation of lobbying, while a similar number advised the abolition of free passes.⁴¹

WHAT IS THE MEANING OF THIS AWAKENING to something that Americans had, in a sense, known about all along? Should we accept the originality of the "discovery" that monied interests endangered free government or lay stress instead on the familiar elements the charge contained? It had, after all, been a part of American political thought since the eighteenth century and had been powerfully repeated, in one form or another, by major and minor figures throughout the nineteenth century. According to Richard Hofstadter, "there was nothing new in the awareness of these things."⁴² In fact, however, there was much that was new. First, many of the details of politico-business corruption had never been publicly revealed before. No one had ever probed the subject as thoroughly as journalists and legislative investigators were now doing, and, moreover, some of the practices they uncovered had only recently come into being. Large-scale corporation campaign contributions, for instance, were a product of the 1880s and 1890s. Highly organized legislative lobbying operations by competing interest groups represented an even more recent development. In his systematic study of American legislative practices, published in 1907, Paul S. Reinsch devoted a lengthy chapter to describing how business interests had developed a new and "far more efficient system of dealing with legislatures than [the old methods of] haphazard corruption."⁴³

supplemented when necessary by newspapers), only two fail to support the generalization given here: Wisconsin, where an awareness of politico-business corruption was demonstrated in the platforms of 1900 and 1902 as well as those of later years; and New Jersey, where the Democrats used the issue sparingly in 1901 and 1904, while the Republicans almost completely ignored it throughout the decade.

⁴¹ New York State Library, *Digest of Governors' Messages* (Albany, N. Y., 1903-09). This annual document, published for the years 1902-08, classifies the contents of the governors' messages by subject and permits easy comparison among them. For Mickey's and Folk's denunciations, see New York State Library, *Digest of Governors' Messages, 1905*, classifications 99 (legislative lobbying), 96 (legislative bribery).

⁴² Hofstadter, *The Age of Reform*, 185.

⁴³ Reinsch, *American Legislatures and Legislative Methods* (New York, 1907), 231. On the history of party campaign funds, see James K. Pollock, Jr., *Party Campaign Funds* (New York, 1926); Earl R. Sikes, *State and Federal Corrupt-Practices Legislation* (Durham, N.C., 1928); and Louise Overacker, *Money in Elections* (New York, 1932).

Even more startling than the new practices themselves was the fresh meaning they acquired from the nationwide character of the patterns that were now disclosed. The point is not simply that more people than ever before became aware of politico-business corruption but that the perception of such a national pattern itself created new political understandings. Lincoln Steffens's autobiography is brilliant on this point. As Steffens acknowledged, much of the corruption he observed in his series on the "shame" of the cities had already come to light locally before he reported it to a national audience. What he did was take the facts in city after city, apply imagination to their transcription, and form a new truth by showing the same process at work everywhere. Here was a solution to the problem the Adams brothers had encountered in writing *Chapters of Erie*: how to report shocking corruption without making it seem too astounding to be representative. The solution was breadth of coverage. Instead of looking at only two businessmen, study dozens; explore city after city and state after state and report the facts to a people who were vaguely aware of corruption in their own home towns but had never before seen that a single process was at work across the country.⁴⁴ This concept of a "process" of corruption was central to the new understanding. Uncovered through systematic journalistic research and probing legislative investigations, corruption was now seen to be the result of concrete historical developments. It could not just be dismissed as the product of misbehavior by "bad" men (although that kind of rhetoric continued too) but had to be regarded as an outcome of identifiable economic and political forces. In particular, corruption resulted from an outmoded policy of indiscriminate distribution, which could not safely withstand an onslaught of demands from private corporations that were larger than the government itself.⁴⁵

Thus in its systematic character, as well as in its particular details, the corruption that Americans discovered in 1905 and 1906 was different from the kind their eighteenth- and nineteenth-century forebears had known. Compared to the eighteenth-century republican understanding, the progressive concept of corruption regarded the monied interests not as tools of a designing administration but as independent agents. If any branch of government was in alliance with them, it was probably the legislature. In a curious way, however, the old republican view that commerce inherently threatened the people's virtue still persisted, now informed by a new understanding of the actual process at work. Compared to Andrew Jackson, the progressives saw big corporations not as monsters but as products of social and industrial development. And their activist remedies differed entirely from his negativistic ones. But, like Jackson, those

⁴⁴ Steffens later commented insightfully on his own (and, by implication, the country's) process of "discovery" during these years; see his *Autobiography*, 357-627. Also see his *Shame of the Cities*, 3-26; and Filler, *The Muckrakers*, 257-59.

⁴⁵ Around 1905 a social-science literature emerged that attempted to explain the process of corruption and to suggest suitable remedies. In addition to Reinsch's *American Legislatures and Legislative Methods*, see Frederic C. Howe, *The City: The Hope of Democracy* (New York, 1905), and *Privilege and Democracy in America* (New York, 1910); and Robert C. Brooks, *Corruption in American Politics and Life* (New York, 1910). Several less scholarly works also analyze the cause of politico-business corruption; see, for example, George W. Berge, *The Free Pass Bribery System* (Lincoln, Neb., 1905); Philip Loring Allen, *America's Awakening: The Triumph of Righteousness in High Places* (New York, 1906); and William Allen White, *The Old Order Changeth: A View of American Democracy* (New York, 1910).

TABLE 1
Selected Categories of State Legislation, 1903-08

<i>Type of Legislation</i>	<i>1903-04</i>	<i>1905-06</i>	<i>1907-08</i>	<i>1903-08</i>
Regulation of Lobbying	0	2	10	12
Prohibition of Corporate Campaign Contributions	0	3	19	22
Regulation or Prohibition of Free Railroad Passes for Public Officials	4	6	14	24
Mandatory Direct Primary	4	9	18	31
Regulation of Railroad Corporations by Commission	5	8	28	41
TOTALS	13	28	89	130

NOTE: Figures represent the number of states that passed legislation in the given category during the specified years.

SOURCE: New York State Library, *Index of Legislation* (Albany, N.Y., 1904-09).

who now discovered corruption grasped that private interests could conflict with the public interest and that government benefits for some groups often hurt others. The recognition of these two things—both painfully at odds with the nineteenth century's conventional wisdom—had been at the root of the floundering over principles of political economy in the 1890s and early 1900s. Now, rather suddenly, the discovery that business corrupts politics suggested concrete answers to a people who were ready for new policies but had been uncertain how to get them or what exactly they should be.

Enacted in a burst of legislative activity immediately following the awakening of 1905 and 1906, the new policies brought to an end the paralysis that had gripped the polity and constituted a decisive break with nineteenth-century patterns of governance. Many states passed laws explicitly designed to curtail illicit business influence in politics. These included measures regulating legislative lobbying, prohibiting corporate campaign contributions, and outlawing the acceptance of free transportation passes by public officials. In 1903 and 1904, there had been almost no legislation on these three subjects; during 1905 and 1906, several states acted on each question; and, by 1907 and 1908, ten states passed lobbying laws, nineteen took steps to prevent corporate contributions, and fourteen acted on the question of passes (see Table 1). If these laws failed to wipe out corporation influence in politics, they at least curtailed important means through which businesses had exercised political power in the late nineteenth and early twentieth centuries. To be sure, other means were soon found, but the flood of state lawmaking on these subjects, together with the corresponding attention they received from the federal government in these same years, shows

how prevalent was the determination to abolish existing forms of politico-business corruption.⁴⁶

Closely associated with these three measures were two more important categories of legislation, often considered to represent the essence of progressivism in the states: mandatory direct primary laws and measures establishing or strengthening the regulation of utility and transportation corporations by commission. These types of legislation, too, reached a peak in the years just after 1905–06, when so many states had experienced a crisis disclosing the extent of politico-business corruption. Like the laws concerning lobbying, contributions, and passes, primary and regulatory measures were brought forth amidst intense public concern with business influence in politics and were presented by their advocates as remedies for that problem. Both types of laws had been talked about for years, but the disclosures of 1905–06 provided the catalyst for their enactment.

Even before 1905, the direct primary had already been adopted in some states. In Wisconsin, where it was approved in 1904, Robert M. La Follette had campaigned for direct nominations since the late 1890s on the grounds that they would “emancipate the legislature from all subserviency to the corporations.” In his well-known speech, “The Menace of the Machine” (1897), La Follette explicitly offered the direct primary as “the remedy” for corporate control of politics. Now, after the awakening of 1905–06, that same argument inspired many states that had failed to act before to adopt mandatory direct primary laws (see Table 1). In New York, Charles Evans Hughes, who was elected governor in 1906 because of his role as chief counsel in the previous year’s life insurance investigation, argued that the direct primary would curtail the power of the special interests. “Those interests,” he declared, “are ever at work stealthily and persistently endeavoring to pervert the government to the service of their own ends. All that is worst in our public life finds its readiest means of access to power through the control of the nominating machinery of parties.” In other states, too, in the years after 1905–06, the direct primary was urged and approved for the same reasons that La Follette and Hughes advanced it.⁴⁷

⁴⁶ The figures in this paragraph (and in the accompanying table) are based on an analysis of the yearly summaries of state legislation reported in New York State Library, *Index of Legislation* (Albany, N.Y., 1904–09). The laws included here are drawn from among those classified in categories 99 (lobbying), 154 (corporate campaign contributions), 1237 (free passes), 160 (direct nominations), and 1267, 1286 (transportation regulation). The legislative years are paired because so many state legislatures met only biennially, usually in the odd-numbered years; no state is counted more than once in any one category in any pair of years. The *Index of Legislation* should be used in conjunction with the accompanying annual *Review of Legislation* (Albany, N. Y., 1904–09).

⁴⁷ Ellen Torelle, comp., *The Political Philosophy of Robert M. La Follette* (Madison, Wisc., 1920), 28; and Hughes, *Public Papers of Charles E. Hughes, Governor, 1909* (Albany, 1910), 37. Also see Maxwell, *La Follette and the Rise of the Progressives*, 13, 27–35, 48–50, 53–54, 74; Allen Fraser Lovejoy, *La Follette and the Establishment of the Direct Primary in Wisconsin, 1890–1904* (New Haven, 1941); Wesser, *Charles Evans Hughes*, 250–301; Direct Primaries Association of the State of New York, *Direct Primary Nominations: Why Voters Demand Them. Why Bosses Oppose Them* (New York, 1909); Ralph Simpson Boots, *The Direct Primary in New Jersey* (New York, 1917), 59–70; Grantham, *Hoke Smith and the Politics of the New South*, 158, 162, 172–73, 178, 193; Schell, *History of South Dakota*, 260; Olin, *California’s Prodigal Sons*, 13; and Charles Edward Merriam and Louise Overacker, *Primary Elections* (Chicago, 1928), 4–7, 60–66.

The creation of effective regulatory boards—progressivism's most distinctive governmental achievement—also followed upon the discovery of politico-business corruption. From 1905 to 1907 alone, fifteen new state railroad commissions were established, and at least as many existing boards were strengthened. Most of the new commissions were "strong" ones, having rate-setting powers and a wide range of administrative authority to supervise service, safety, and finance. In the years to come, many of them extended their jurisdiction to other public utilities, including gas, electricity, telephones, and telegraphs. Direct legislative supervision of business corporations was also significantly expanded in these years. Life insurance companies—whose corruption of the New York State government Hughes had dramatically disclosed—provide one example. "In 1907," as a result of Hughes's investigation and several others conducted in imitation of it, Morton Keller has reported, "forty-two state legislatures met; thirty considered life insurance legislation; twenty-nine passed laws. . . . By 1908 . . . [the basic] lines of twentieth century life insurance supervision were set, and thereafter only minor adjustments occurred." The federal regulatory machinery, too, was greatly strengthened at this time, most notably by the railroad, meat inspection, and food and drug acts of 1906.⁴⁸

The adoption of these measures marked the moment of transition from a structure of economic policy based largely on the allocation of resources and benefits to one in which regulation and administration played permanent and significant roles. Not confined for long to the transportation, utility, and insurance companies that formed its most immediate objects, regulatory policies soon were extended to other industries as well. Sometimes the legislative branch took responsibility for the ongoing tasks of supervision and administration, but more commonly they became the duty of independent boards and commissions, staffed by experts and entrusted with significant powers of oversight and enforcement. Certainly, regulation was not previously unknown, nor did promoting commerce and industry now cease to be a governmental purpose. But the middle years of the first decade of the twentieth century unmistakably mark a turning point—that point when the direction shifted, when the weight of opinion changed, when the forces of localism and opposition to governmental authority that had sustained the distribution of privileges but opposed regulation and administration now lost the upper hand to the forces of centralization, bureaucratization, and government actions to recognize and adjust group differences. Besides economic regulation, other governmental policy areas, including health, education, taxation, correction, and the control of natural resources, increasingly came under the jurisdiction of independent boards and commissions. The establishment of these agencies and the expansion of their duties meant

⁴⁸ Huebner, "Five Years of Railroad Regulation by the States"; Robert Emmett Ireton, "The Legislatures and the Railroads," *Review of Reviews*, 36 (1907): 217–20; and Keller, *The Life Insurance Enterprise, 1885–1910: A Study in the Limits of Corporate Power* (Cambridge, Mass., 1963), 257, 259. The manner in which the states copied each other's legislation in this period is a subject deserving of study; for a suggestive approach, see Jack L. Walker, "The Diffusion of Innovations among the American States," *APSR*, 63 (1969): 880–99.

that American governance in the twentieth century was significantly different from what it had been in the nineteenth.⁴⁹

The developments of 1905–08 also changed the nature of political participation in the United States. Parties emerged from the years of turmoil altered and, on balance, less important vehicles of popular expression than they had been. The disclosures of politico-business wrongdoing disgraced the regular party organizations, and many voters showed their loss of faith by staying at home on election day or by casting split tickets. These trends had been in progress before 1905–06—encouraged by new election laws as well as by the crisis of confidence in traditional politics and government—but in several ways the discovery of corruption strengthened them. Some reigning party organizations were toppled by the disclosures, and the insurgents who came to power lacked the old bosses' experience and inclination when it came to rallying the electorate. And the legal prohibition of corporate campaign contributions now meant, moreover, that less money was available for pre-election entertainment, transportation to the polls, and bribes.⁵⁰

While the party organizations were thus weakened, they were also more firmly embedded in the legal machinery of elections than ever before. In many states the direct primary completed a series of new election laws (beginning with the Australian ballot in the late 1880s and early 1890s) that gave the parties official status as nominating bodies, regulated their practices, and converted them into durable, official bureaucracies. Less popular now but also more respectable, the party organizations surrendered to state regulation and relinquished much of their ability to express community opinion in return for legal guarantees that they alone would be permanently certified to place nominees on the official ballot.⁵¹

Interest organizations took over much of the parties' old job of articulating

⁴⁹ Among the best accounts of this transformation in policy are Herbert Croly, *Marcus Alonzo Hanna: His Life and Work* (New York, 1912), 465–79; Hurst, *Law and the Conditions of Freedom*, 71–108; and Wiebe, *The Search for Order, 1877–1920*, 164–95.

⁵⁰ The causes of the decline in party voting have been the subject of considerable debate and disagreement among political scientists and historians in recent years. Walter Dean Burnham began the controversy when he first described the early-twentieth-century changes in voting behavior and explained them by suggesting that an antipartisan industrial elite had captured the political system after the realignment of the 1890s; "Changing Shape of the American Political Universe." Jerrold G. Rusk and Philip E. Converse responded by contending that legal-institutional factors could better account for the behavioral changes that Burnham had observed; Rusk, "The Effect of the Australian Ballot Reform on Split Ticket Voting"; and Converse, "Change in the American Electorate," in Angus Campbell and Philip E. Converse, eds., *The Human Meaning of Social Change* (New York, 1972), 263–337. All three political scientists carried the debate forward—and all withdrew a bit from their original positions—in the September 1974 issue of the *American Political Science Review*. At present, the weight of developing evidence seems to indicate that, while new election laws alone cannot explain the voters' changed behavior, Burnham's notion of an elite takeover after 1896 is also inadequate to account for what happened; McCormick, *From Realignment to Reform*, chap. 9. What I am suggesting here is that the shock given to party politics by the awakening of 1905–06 played an important part in solidifying the new tendencies toward lower rates of voter participation and higher levels of ticket splitting. On the relative scarcity of campaign funds in the election of 1908, see Pollock, *Party Campaign Funds*, 37, 66–67; Overacker, *Money in Elections*, 234–38; and Brooks, *Corruption in American Politics and Life*, 234–35.

⁵¹ Peter H. Argersinger, "'A Place on the Ballot': Fusion Politics and Antifusion Laws," *AHR*, 85 (1980): 287–306; Merriam and Overacker, *Primary Elections*; and William Mills Ivins, *On the Electoral System of the State of New York* (Albany, 1906).

popular demands and pressing them upon the government. More exclusive and single-minded than parties, the new organizations became regular elements of the polity. Their right to represent their members before the government's new boards and agencies received implicit recognition, and, indeed, the commissions in some cases became captives of the groups they were supposed to regulate. The result was a fairly drastic transformation of the rules of political participation: who could compete, the kinds of resources required, and the rewards of participation all changed. These developments were not brand new in the first years of the twentieth century, but, like the contemporaneous changes in government policy, they derived impressive, decisive confirmation from the political upheaval that occurred between 1905 and 1908.

POLITICAL AND GOVERNMENTAL CHANGES thus followed upon the discovery that business corrupts politics. And Americans of the day explicitly linked the two developments: the reforms adopted in 1907-08 were to remedy the ills uncovered in 1905-06. But these chronological and rhetorical connections between discovery and reform do not fully explain the relationship between them. Why, having paid relatively little heed to similar charges before, did people now take such strong actions in response to the disclosures? Why, moreover, did the perception of wrongdoing precipitate the particular pattern of responses that it did—namely, the triumph of bureaucracy and organization? Of most importance, what distinctive effects did the discovery of corruption have upon the final outcome of the crisis?

By 1905 a political explosion of some sort was likely, due to the accumulated frustrations people felt about the government's failure to deal with the problems of industrialization. So combustible were the elements present that another spark besides the discovery of politico-business corruption might well have ignited them. But the recognition of such corruption was an especially effective torch. Upon close analysis, its ignition of the volatile political mass is unsurprising. The accusations made in 1905-06 were serious, widespread, and full of damaging information; they explained the actual corrupt process behind a danger that Americans had historically worried about, if not always responded to with vigor; they linked in dark scandal the two main villains—party bosses and big businessmen—already on the American scene; they inherently discredited the existing structure of economic policy based on the distribution of privileges; and they dramatically suggested the necessity for new kinds of politics and government. That businessmen systematically corrupted politics was incendiary knowledge; given the circumstances of 1905, it could hardly have failed to set off an explosion.

The organizational results that followed, however, seem less inevitable. There were, after all, several other known ways of curtailing corruption besides expert regulation and administration. For one, there was the continued reliance on direct legislative action against the corruption of politics by businessmen. The lobbying, anti-free pass, and campaign-contribution measures of 1907-08 ex-

emplified this approach. So did the extension of legislative controls over the offending corporations. Such measures were familiar, but obviously they were considered inadequate to the crisis at hand. A second approach, favored by Edward Alsworth Ross and later by Woodrow Wilson, was to hold business leaders personally responsible for their "sins" and to punish them accordingly. There were a few attempts to bring individuals to justice, but, because of the inadequacy of the criminal statutes, the skill of high-priced lawyers, and the public's lack of appetite for personal vendettas, few sinners were jailed. Finally, there were proposals for large structural solutions changing the political and economic environment so that the old corrupt practices became impossible. Some men, like Frederic C. Howe, still advocated the single tax and the abolition of all privileges granted by government.⁵² Many more believed in the municipal ownership of public utilities. Hundreds of thousands (to judge from election returns) favored socialist solutions, but most Americans did not. In their response to politico-business corruption, they went beyond existing legislative remedies and avoided the temptation to personalize all the blame, but they fell short of wanting socialism, short even of accepting the single tax.

Regulation and administration represented a fourth available approach. Well before the discoveries of 1905-06, groups who stood to benefit from governmental control of utility and transportation corporations had placed strong regulatory proposals on the political agendas of the states and the nation. In other policy areas, the proponents of an administrative approach had not advanced that far prior to 1905-06, but theirs was a large and growing movement, supported—as recent historians have shown—by many different groups for varied, often contradictory, reasons.⁵³ The popular awakening to corruption increased the opportunity of these groups to obtain enactment of their measures. Where their proposals met the particular political needs of 1905-08, they succeeded most quickly. Regulation by commissions seemed to be an effective way to halt corruption by transferring the responsibility for business-government relations from party bosses and legislators to impartial experts. That approach also possessed the additional political advantages of appearing sane and moderate, of meeting consumer demands for government protection, and, above all, of being sufficiently malleable that a diversity of groups could be induced to anticipate favorable results from the new policies.⁵⁴

In consequence, the passions of 1905-06 added support to an existing movement toward regulation and administration, enormously speeded it up, shaped

⁵² Ross, *Sin and Society: An Analysis of Latter-Day Iniquity* (Boston, 1907); John M. Blum, *Woodrow Wilson and the Politics of Morality* (Boston, 1956); John B. Roberts, "The Real Cause of Municipal Corruption," in Clinton Rogers Woodruff, ed., *Proceedings of the New York Conference for Good City Government*, National Municipal League publication (Philadelphia, 1905), 148-53; and Howe, *Privilege and Democracy in America*.

⁵³ For an astute analysis of which groups favored and which groups opposed federal railroad legislation, see Richard H. K. Vietor, "Businessmen and the Political Economy: The Railroad Rate Controversy of 1905," *JAH*, 64 (1977): 47-66; and, for an excellent survey of the literature on regulation, see Thomas K. McCraw, "Regulation in America: A Review Article," *Business History Review*, 49 (1975): 159-83. The best account of the emergence of administrative ideas is, of course, Wiebe, *The Search for Order, 1877-1920*, 133-95.

⁵⁴ On the adaptability of administrative government, see Otis L. Graham, Jr., *The Great Campaigns: Reform and War in America, 1900-1928* (Englewood Cliffs, N.J., 1971), 50-51; and Wiebe, *The Search for Order, 1877-1920*, 222-23, 302.

the timing and form of its victory, and probably made the organizational revolution more complete—certainly more sudden—than it otherwise would have been. These accomplishments alone must make the discovery of corruption pivotal in any adequate interpretation of progressivism. But the awakening did more than hurry along a movement that already possessed formidable political strength and would probably have triumphed eventually even without the events of 1905–06. By pushing the political process toward so quick a resolution of the long-standing crisis over industrialism, the passions of those years caused the outcome to be more conservative than it otherwise might have been. This is the ultimate irony of the discovery that business corrupts politics.

Muckraking accounts of politico-business evils suggest one reason for the discovery's conservative impact. Full of facts and revelations, these writings were also dangerously devoid of effective solutions. Charles E. Russell's *Lawless Wealth* (1908)—the title itself epitomizes the perceptions of 1905–06—illustrates the flaw. Published originally in *Everybody's Magazine* under the accusatory title, "Where Did You Get It, Gentlemen?," the book recounts numerous instances of riches obtained through the corruption of politics but, in its closing pages, merely suggests that citizens recognize the evils and be determined to stop them. This reliance on trying to change how people felt (to "shame" them, in Steffens's phrase) was characteristic of muckraking and of the exposures of 1905–06. One can admire the muckrakers' reporting, can even accept David P. Thelen's judgment that their writing "contained at least as deep a moral revulsion toward capitalism and profit as did more orthodox forms of Marxism," yet can still feel that their proposed remedy was superficial. Because the perception of politico-business corruption carried no far-reaching solutions of its own or genuine economic grievances, but only a desire to clean up politics and government, the passions of 1905–06 were easily diverted to the support of other people's remedies, especially administrative answers. Had the muckrakers and their local imitators penetrated more deeply into the way that business operated and its real relationship to government, popular emotions might not have been so readily mobilized in support of regulatory and administrative agencies that business interests could often dominate. At the very least, there might have been a more determined effort to prevent the supervised corporations themselves from shaping the details of regulatory legislation. Thus, for all of their radical implications, the passions of 1905–06 dulled the capacity of ordinary people to get reforms in their own interest.⁵⁵

The circumstances in which the discovery of corruption became a political force also assist in explaining its conservatism. The passions of 1905–06 were primarily expressed in state, rather than local or national, politics. Indeed, those passions often served to shift the focus of reform from the cities to the state capitals. There—in Albany, or Madison, or Sacramento—the remedies were worked out in relative isolation from the local, insurgent forces that had in many cases

⁵⁵ Russell, *Lawless Wealth: The Origin of Some Great American Fortunes* (New York, 1908), 30–35, 52–55, 274–79; and Thelen, "Lincoln Steffens and the Muckrakers: A Review Essay," *Wisconsin Magazine of History*, 58 (1975): 316.

originally called attention to the evils. Usually the policy consequences were more favorable to large business interests than local solutions would have been. State utility boards, for example, which had always been considered more conservative in their policies than comparable local commissions, now took the regulatory power away from cities and foreclosed experimentation with such alternatives as municipal ownership or popularly chosen regulatory boards. In gaining a statewide hearing for reform, the accusations of politico-business corruption actually increased the likelihood that conservative solutions would be adopted.⁵⁶

Considering the intensity of the feelings aroused in 1905 and 1906 ("the wrath of thousands of private citizens . . . is at white heat over the disclosures," declared a Rochester newspaper) and the catalytic political role they played, the awakened opposition to corruption was surprisingly short-lived. As early as 1907 and 1908, the years of the most significant state legislative responses to the discovery, the messages of the governors began to exhibit a more stylized, less passionate way of describing politico-business wrongdoing. Now the governors emphasized remedies rather than abuses, and most seemed confident that the remedies would work. Criticism of business influence in government continued to be a staple of political rhetoric throughout the Progressive era, but it ceased to have the intensity it did in 1905–06. In place of the burning attack on corruption, politicians offered advanced progressive programs, including further regulation and election-law reforms.⁵⁷ The deep concern with business corruption of politics and government thus waned. It had stirred people to consciousness of wrongdoing, crystalized their discontent with existing policies, and pointed toward concrete solutions for the ills of industrialism. But it had not sustained the more radical, antibusiness possibilities suggested by the discoveries of 1905–06.

Indeed, the passions of those years probably weakened the insurgent, democratic qualities of the ensuing political transformation and strengthened its bureaucratic aspects. This result was ironical, but its causes were not conspiratorial. They lay instead in the tendency—shared by the muckrakers and their audience—to accept remedies unequal to the problems at hand and in political circumstances that isolated insurgents from decision making. Once the changes in policy were under way after 1906, those organized groups whose interests were most directly affected entered the fray, jockeyed for position, and heavily shaped the outcomes. We do not yet know enough about how this happened, but studies such as Stanley P. Caine's examination of railroad regulation in Wisconsin suggest how difficult it was to translate popular concern on an "issue" into the details of a law.⁵⁸ It is hardly surprising that, as regulation and adminis-

⁵⁶ Nord, "The Experts versus the Experts"; and Thelen, *Robert M. La Follette and the Insurgent Spirit*, 50–51.

⁵⁷ *Rochester Democrat and Chronicle*, October 18, 1905; and New York State Library, *Digest of Governors' Messages, 1907, 1908*. In a number of states where politico-business corruption had been an issue in the party platforms around 1906, the platforms were silent on the subject by 1910.

⁵⁸ Caine, *The Myth of a Progressive Reform: Railroad Regulation in Wisconsin, 1903–1910* (Madison, Wisc., 1970), 70. Also see Mansel G. Blackford, *The Politics of Business in California, 1890–1920* (Columbus, Ohio, 1977); Bruce W. Dearstyne, "Regulation in the Progressive Era: The New York Public Service Commission," *New York History*, 58 (1977): 331–47; and McCraw, "Regulation in America." These and other studies cast considerable doubt on the applicability at the state level of Gabriel Kolko's interpretation of regulatory legislation;

tration became accepted public functions, the affected interests exerted much more influence on policy than did those who cared most passionately about restoring clean government.

But the failure to pursue antibusiness policies does not mean the outcry against corruption was either insincere or irrelevant. Quite the contrary. It was sufficiently genuine and widespread to dominate the nation's public life in 1905 and 1906 and to play a decisive part in bringing about the transformation of American politics and government. Political changes do not, of course, embrace everything that is meant by progressivism. Nor was the discovery that business corrupts politics the only catalytic agent at work; certainly the rise of consumer discontent with utility and transportation corporations and the vigorous impetus toward new policies given by Theodore Roosevelt during his second term as president played complementary roles. But the awakening to corruption—as it was newly understood—provided an essential dynamic, pushing the states and the nation toward what many of its leading men and women considered progressive reform.

The organizational thesis sheds much light on the values and methods of those who succeeded in dominating the new types of politics and government but very little on the political circumstances in which they came forward. Robert H. Wiebe, in particular, has downplayed key aspects of the political context, including the outcry against corruption. Local uprisings against the alliance of bosses and businessmen, Wiebe has stated, “lay outside the mainstream of progressivism”; measures instituting the direct primary and curtailing the political influence of business were “old-fashioned reform.”⁵⁹ Yet those local crusades, by spreading the dynamic perception that business corrupts politics, created a popular demand for the regulatory and administrative measures that Wiebe has claimed are characteristic of true progressivism; and those “old-fashioned” laws were enacted amidst the same political furor that produced the stunningly rapid bureaucratic triumph whose significance for twentieth-century America Wiebe has explained so convincingly. What the organizational thesis mainly lacks is the sense that political action is open-ended and unpredictable. Consequences are often unexpected, outcomes surprising when matched against origins. While it is misleading, as Samuel P. Hays has said, to interpret progressivism solely on the basis of its antibusiness ideology, it is equally misleading to fail to appreciate that reform gained decisive initial strength from ideas and feelings that were not able to sustain the movement in the end.⁶⁰ The farsighted organizers from business and the professions thus gained the opportunity to complete a political transformation that had been begun by people who were momentarily shocked into action but who stopped far short of pursuing the full implications of their discovery.

for that position, see his *The Triumph of Conservatism*. Commonly, the affected interests opposed state regulation until its passage became inevitable, at which point they entered the contest in order to influence the details of the law. Businessmen often had considerable, but not complete, success in helping shape such legislation, and they frequently found it beneficial in practice.

⁵⁹ Wiebe, *The Search for Order, 1877–1920*, 172, 180.

⁶⁰ Hays, “The Politics of Reform in Municipal Government.”

Rome, Constantinople, and the Barbarians

WALTER GOFFART

IF THE PHYSICAL LAW OF INERTIA applies to historical developments, then perhaps the Roman Empire was legitimately destined for eternity, and those who know that it did not endure are bound to ask what interrupted its tranquil course through the ages. On a superficial level, there is no mystery. Almost everyone agrees on what it was that turned Rome in unexpected directions. Edward Gibbon said that his narrative of decline and fall described "the triumph of barbarism and superstition"; Arnold Toynbee's modernized version of the same phrase attributed the fall to "the 'internal' and 'external' proletariat."¹ These provocative formulations have a neutral alternative. No one seriously doubts that the Roman Empire in its final phase was most profoundly affected, on the one hand, by the Christian religion and, on the other, by those foreign tribes generally called "the barbarians." If we wish to understand not just the fall of Rome but also the opening of the Middle Ages, we have to come to terms with these separate and highly complex phenomena.

Only the barbarians will be considered here. As Gibbon implied, Toynbee affirmed, and everyone else widely believes, they epitomized the "external" dimension of the fall of the empire. This perception is obviously true inasmuch as barbarians are, by definition, foreigners. Yet to acknowledge the ethnic or cultural distinctiveness of barbarians is not necessarily to maintain—as many historians have tended to do in recent years²—that the Roman Empire, or part of

The immediate ancestors of this article are a talk given at the Conference on Medieval Studies, held at Western Michigan University, Kalamazoo, in May 1978, and its fuller development into a lecture given at the University of Arizona, Tucson, in March 1979. Other antecedents, with thankful acknowledgments that also apply here, are spelled out in my *Barbarians and Romans (A.D. 418–584): The Techniques of Accommodation* (Princeton, 1980). This article was written while I held a Guggenheim Fellowship. Extracts from it were delivered as lectures at the Universities of Cambridge, Oxford, and Leeds in February and May 1980.

¹ Gibbon, *History of the Decline and Fall of the Roman Empire*, ed. J. B. Bury, 7 (London, 1909): 308; and Toynbee, *A Study of History*, 1 (London, 1934): 41.

² André Piganiol, *L'Empire chrétien* (Paris, 1947), 421–22; Émilienne Demougeot, *De l'unité à la division de l'Empire romain, 395–410* (Paris, 1951), 566–70; A. R. Hands, "The Fall of the Roman Empire in the West: A Case of Suicide or Force majeure?" *Greece and Rome*, 2d ser., 10 (1963): 153–68; A. H. M. Jones, *The Later Roman Empire* (Oxford, 1964), 1027–31; B. H. Warmington, Review of *ibid.*, in *History*, 50 (1965): 57–58; and J. F. Matthews, Review of Massimiliano Pavan, *La Politica Gotica di Teodosio nella pubblicista del suo tempo* (Rome, 1964), in the *Journal of Roman Studies*, 56 (1966): 245. Matthews has stated the point succinctly: "Gothic pressure on the frontiers was in the long run irresistible." As historians of Rome have turned their attention increasingly to late antiquity, the easy denigrations of the age have been replaced by positive assessments, pioneered by art historians but now applied, as by Jones, to social and economic history as well. If the empire is judged not to have been internally "sick," one is bound to bring back to prominence the "ruin which comes from outside."

it, was overcome by pressure from outside its borders. The dualism of internal and external causation has its classic statement in Polybius's meditation on the fall of states, written in the second century B.C.: "And it is also all too evident that ruin and change are hanging over everything. The necessity of nature is enough to convince us of this. Now there are two ways in which any type of state may die. One is the ruin which comes from outside; the other, in contrast, is the internal crisis. The first is difficult to foresee, the second is determined from within."³ The latter had sole claim to Polybius's analytical skills, leaving it for us to ask whether barbarians of the Christian era, like the Goths, Vandals, Huns, and so forth, may be adapted to his idea of an unforeseeable "ruin which comes from outside." Although Polybius did not give concrete examples, much later incidents of unexpected calamity come readily to mind—most notably, the arrival of the *conquistadores* in America. No one would suggest, however, that what Rome experienced in late antiquity bore any resemblance to the fate of the Aztecs and Incas. The barbarian invasions definitely did not happen to an unsuspecting empire, as though mysterious beings had landed from outer space. On the contrary, Rome had always had warlike tribesmen at its gates and had centuries of experience in dealing with them.

Polybius is, perhaps, an inappropriate guide. A less famous historian—a late Roman familiar with the Old Testament as well as with the themes of ancient historiography—was able to evoke a pattern in which aliens play a passive but crucial part in a downfall "determined from within." This author was Sulpicius Severus, whose compendium of Hebrew history (more rarely read than his life of St. Martin) is full of object lessons for the Christian Romans of the turn of the fifth century.⁴ A brief extract, based on Judges 1 and 3, contains the essentials.

Under [the] guidance [of Judah], matters were successfully conducted: there was the greatest tranquility both at home and abroad. . . . Then, as almost always happens in a time of prosperity, [the Hebrews] began to contract marriages from among the conquered, and by and by to adopt foreign customs, yea, even in a sacrilegious manner to offer sacrifices to idols: so pernicious is all alliance with foreigners. God, foreseeing these things long before, had, by a wholesome precept, enjoined upon the Hebrews to give over the conquered nations to utter destruction. But the people, through lust for power, preferred (to their own ruin) to rule over those who were conquered. Accordingly, when, forsaking God, they worshipped idols, they were deprived of divine assistance and, being vanquished and subdued by the king of Mesopotamia, they paid the penalty of eight years' captivity.⁵

Though crude and xenophobic, the passage has the advantage of portraying a "ruin" that Polybius never imagined. Sulpicius presented a drama whose motive force is provided not by the aliens as such but by the Hebrews' typically "imperial" relations to them. The role played by foreigners, however objection-

³ Polybius 6. 57. 1–2, as quoted in Santo Mazzarino, *The End of the Ancient World*, trans. George Holmes (London, 1966), 23.

⁴ For notable examples of such lessons, see *Chronica* 2. 3. 5, 1. 32. 3.

⁵ *Chronica* 1.24. 1, in Alexander Roberts, trans., *Select Library of Nicene and Post-Nicene Fathers*, 2d ser., 11 (New York, 1894): 82.

able Sulpicius's account of it may be, is as much an internal one as if, instead, the strife of domestic factions had undermined the state. Should we not have something like this in mind when thinking of Rome and the barbarians?

MORE THAN POETIC CONVENTION was involved in the practice that late ancient authors adopted of portraying the tribes of their time under anachronistic names drawn from Herodotus and Tacitus. Disguising the Goths as Getae or Scythians, the Franks as Sicambri, and the Huns as Massagetae expressed the underlying truth that there had been no change of substance beyond the frontiers. The turbulent tribes of yesteryear prolonged their existence under new names; as they had once been kept in check, so could they be today.⁶ The Celts who had captured Rome in 390 B.C. and burned it down, Hannibal and the Carthaginians, the redoubtable Mithridates of Pontus, Ariovistus and Vercingetorix in Gaul—all these and many more were no less barbarian than the Dacians of the high empire and the Goths and Huns in the 370s. Precisely because the barbarians were always there, never seeming to contemporary observers from the Mediterranean to acquire new characteristics more dangerous than those of the past, there is little reason to look among them for a clue to their startling career in the fifth and sixth centuries A.D. The changes in their relations to the Roman Empire need to be examined from the Roman side of the border, for it was on that side—not least because the literate observers were there—that the terms of the encounter were formulated and the dynamics governing the relations of the parties almost invariably generated.

The term barbarian itself is, as everyone knows, a Greco-Roman generalization. It began as the Greek name for all of those who did not speak Greek, and, with somewhat altered meaning, it survived its encounter with Goths, Franks, and Saxons to win a secure place in the vocabularies of medieval and modern Europe.⁷ Similar words for “foreigner” and “alien,” with similarly negative associations, are common to many tongues.⁸ Human beings and groups inevitably look upon their neighbors with suspicion and distaste and ascribe unflattering characteristics to them.

What is worrisome about the name “barbarian” is the generalization it embodies: the term tends to transform the neighbors of the Roman Empire into a collectivity. In a few contexts, that idea is unobjectionable. When addressing the emperor ca. A.D. 370, a writer on military affairs said, “The first thing to know is that the madness of the nations lurking about everywhere surrounds the Ro-

⁶ The most explicit expression of this sense of continuity occurs in Synesius of Cyrene (ca. A.D. 400), *Discours sur la royauté*, trans. Charles Lacombrade (Paris, 1951), 66. Goths appear as Scythians, Getae, and Massagetae in Synesius, Franks as Sicambri in Claudian, Sidonius Apollinaris, and Gregory of Tours, and Huns as Massagetae in Procopius. There are other instances.

⁷ Karl Christ, “Römer und Barbaren in der hohen Kaiserzeit,” *Saeculum*, 10 (1959): 273–80; and Lieven van Acker, “Barbarus und seine Ableitungen im Mittellatein,” *Archiv für Kulturgeschichte*, 47 (1965): 125–40. For a less satisfactory discussion, see W. R. Jones, “The Image of the Barbarian in Medieval Europe,” *Comparative Studies in Society and History*, 13 (1971): 376–407.

⁸ For example, the Old English *wealh*, whence Welsh, and West Slavic *němec* (pronounced “nyemets”), whence the Slavic name for Germany.

man Empire, and treacherous barbarity, concealed by advantageous terrain, assails every side of the frontiers." Except for a note of paranoia, there is nothing wrong with statements of this kind or with the boast of the emperors whose inscriptions proclaimed them to be victors over "all the barbarians."⁹ We moderns, however, do not occupy the same lookout platform and need, therefore, to remind ourselves often of the limits of the word. Barbarians were a collectivity only when seen from a Greco-Roman or Mediterranean perspective. Even Romans were astute enough to realize how diverse were the *gentes* embraced by that name. The many populations bordering the Roman Empire had their separate labels, as well as their own customs, languages, and traditions. They included the Irish and Picts of the British Isles, the highly civilized Persians on the Syrian frontier, the Berbers of North Africa, Asiatic nomads like the Sarmatians, and many more. An early fourth-century compendium of the Roman provinces—the so-called Verona list—reminds us of this diversity when it complements the catalogue of provinces with one of "the barbarian peoples who multiplied under the emperors." The compiler sensed that a sketch of the empire was somehow incomplete without these nearby foreigners, and he did not fail to recognize that some of them, such as the Isaurians, were natives of lands that had long been integrated in the empire.¹⁰

In most narratives, the barbarians of the invasion period are identified as a matter of course with the Germanic peoples. However correct this identification may be in a scheme of linguistic classification, the collectivity of Germans is a historical anachronism if transposed to the sixth century or earlier. The only collectivities then in existence were particular tribes called Saxons, Alamans, Goths, Herules, and so forth.¹¹ In A.D. 98, a description of the Germans and their lands and customs was written, but its author, typically, was a learned Roman with a trained ethnographer's ability to classify and generalize. No Vandal, Burgundian, or Gepid read Tacitus's famous monograph or otherwise acquired a sense of cross-tribal kinship.¹² Although the name "German" was comparatively widespread in Latin writings of late antiquity, it applied exclusively to the Rhine peoples, notably the Franks. Tribes from the lower Dan-

⁹ On such imperial inscriptions, peculiar to late antiquity, see Christ, "Römer und Barbaren," 281; for the military author, *De rebus bellicis* 6. 1, ed. E. A. Thompson as *A Roman Reformer and Inventor* (Oxford, 1952), 113. The anonymous author's paranoia has an unintended modern counterpart in Owen Lattimore's idea that "excluded barbarians" know much more about the (civilized) peoples "who ejected them" than vice versa, "study [their] strength and weakness in order to break the barrier," and seize the first opportunity to take the offensive; Lattimore, "La Civilisation, mère de barbarie," *Annales: Économies, sociétés, civilisations*, 17 (1962): 107. By the criterion of surviving evidence, there is every reason to think that the Roman Empire was much better informed about neighboring peoples than these "barbarians" were about Rome. Is not such a pattern more normal than the one Lattimore imagined?

¹⁰ Alexander Riese, ed., *Geographi Latini minores* (Heilbronn, 1878), 128–29. The tribes are named one by one, in a catalogue that runs roughly from the North Sea to Mauretania via the Euphrates.

¹¹ See František Graus, *Volk, Herrscher, und Heiliger im Reich der Merowinger* (Prague, 1965), 23–24. For linguistic and other modern perspectives on the term "German," see Rolf Hachmann, *The Germanic Peoples*, trans. James Hogarth (London, 1971), 11–49.

¹² Francis Haverfield, "Tacitus during the Late Roman Period and the Middle Ages," *Journal of Roman Studies*, 6 (1916): 195–200.

ube, like the Visigoths, though Germanic in speech, would never have dreamt of applying this name to themselves.¹³

So much modern writing implies or presupposes a homogeneous Germanic identity that the disunity of the early Germans can hardly be too emphatically stressed. The desire of recent Germans to believe in their antiquity has been so great that even cautious statements of theirs about early conditions have tended to embody wishful thinking. An appropriate example comes from the historians Johannes Haller and Heinrich Dannenbauer: "As betrayed by the lack of a collective name, the Germans did not conceive of themselves as a unity, but they always [considered themselves] related. Thus they knew of a common descent that they traced back to the earthborn god Twist (Zwitter) and his son Mann."¹⁴ This observation would be correct only if the belief in a common descent came directly out of Germania and expressed a thought in the minds of those tribesmen who, in the fifth and sixth centuries, established themselves on Roman soil. The source, however, is Tacitus, whose information is radically limited in space and time. Whatever the situation was in his day, not a shred of evidence from the age of the invasions intimates that the tribes of that era thought of themselves as descendants of a common ancestor.¹⁵ The first faint trace of a "German" consciousness—a sense of kinship among a variety of Germanic peoples—begins to be discernible only in the ninth century—that is, in the Carolingian era.¹⁶ Even then, it was a highly learned idea, not a sentiment rooted in popular consciousness. When the last centuries of antiquity are evoked, the extreme fragmentation of the barbarians should never be overlooked. Even among those speaking similar dialects, there were many hatreds and rivalries; the tribes were at least as ready to cooperate with Romans against their neighbors as they were to join with the latter to make an incursion across the imperial frontiers. At no time in antiquity, early or late, was there a collective hostility of barbarians toward the empire or a collective purpose to tear it down.

The tiresome repetitiveness of Roman relations with the barbarians can, from a modern standpoint, be regarded as a problem in itself. Why is it that, as the

¹³ Hachmann, *The Germanic Peoples*, 29, 49.

¹⁴ Haller and Dannenbauer, *Der Eintritt der Germanen in die Geschichte* (4th ed., Berlin, 1970), 18. For one instance out of very many in which the consciousness of Germanic unity is imprudently exaggerated, see Frank M. Stenton, *Anglo-Saxon England* (3d ed., Oxford, 1971), 192.

¹⁵ Tacitus, *Germania* 2. The contrary was once believed, on the basis of the so-called "Frankish Table of Peoples," but this text has long been recognized to derive information from Tacitus; see, for example, J. Friedrich, "Die sogenannte fränkische Völkertafel," *Sitzungsberichte der bayerische Akademie der Wissenschaften, Philol.-historische Classe* (Munich, 1910), no. 11. Reinhard Wenskus cited only books published in 1940 and seems to have forgotten what regime ruled Germany in that year, in affirming "Freilich ist das Bewusstsein der Gemeinsamkeit der germanische gentes wohl nie völlig erloschen"; Wenskus, "Die deutschen Stämme im Reiche Karls des Grossen," in Wolfgang Braunsfels, ed., *Karl der Grosse: Lebenswerk und Nachleben*, 1 (Düsseldorf, 1965): 190, 196, nn. 102, 108, 168. Anyone who claims that a sense of early Germanic community never "died out" must first establish when it might possibly have burned brightly, let alone been kindled.

¹⁶ Erich Zöllner, *Die politische Stellung der Völker im Frankenreich* (Vienna, 1950), 46–47, 52. The situation in the century before the Carolingians is well illustrated by Isidore of Seville, in whose classifications the Goths, Gepids, Lombards, Saxons, and Franks are definitely not Germans, the Burgundians are foreigners forcibly settled in Germany by the emperor Tiberius, and the *Germanicae gentes* are documented by ancient tribal names, of which only one (the Suevi) had anything to do with the era of the invasions; Isidore, *Etymologiae* 9. 2. 97–101.

fourth century began, the Roman Empire was still more or less surrounded by turbulent, untamed tribesmen? Rather odd statements on this subject have been made in the last decades. A French historian, soon after World War II, unconsciously adopted the tones of an embittered colonial administrator: "The Germans inhabited dreadful lands whose soil they were too lazy to clear. They preferred war to organized work and invaded neighboring states 'pressed by hunger.' Neither the influence of Greece nor that of Rome had succeeded, after so many centuries, in civilizing them." Another scholar believed he had identified the flaw in Roman society that explained why the empire could never win an "ultimate victory" over the barbarians, as though such a victory could have been won, or ought to have been.¹⁷ A healthier approach, perhaps, is to recognize that, in the imperial scheme of things, the barbarian problem was so structured as to be interminable. Outsiders were to be kept clear of the provinces, nothing more. The Roman military frontier could move forward, stand still, or retreat; in any eventuality, there would always be more or less hostile aliens on the other side, because the frontier existed for no other reason than to contain them. It has been suggested that the treaties sometimes made by Rome with neighboring peoples were a step in their "progressive assimilation," but the existence of any imperial plan or intention to assimilate outsiders is highly doubtful. The treaties were simply a means of defense or a preparation for expansion—in either case a useful complement to military action.¹⁸

It is not as though Rome lacked powers of assimilation. In the many centuries since it had entered upon the conquest of distant lands, millions of barbarians had been pacified and absorbed into a common civilization, a Romania whose component peoples, however imperfectly homogeneous, looked to the emperor for defense against outsiders and had no desire for liberation from his rule.¹⁹ Though massive and imposing, assimilation had taken place only inside the zone encompassed by the Roman armies. Submitting or being conquered had traditionally been the condition for participating in the benefits Roman rule had to offer. Thus, the progressive internal development of the empire took place against a backdrop that consisted of an unchangeably barbarous exterior. The never-ending savagery, deceitfulness, and turbulence of barbarians bore witness to the virtues of legally ordered society; their existence justified the imperial regime as the hand that staved off chaos from engulfing the ordered world. While *barbaricum*, aggressive and disorganized, waited to be conquered, its

¹⁷ Piganiol, *L'Empire chrétien*, 420; and E. A. Thompson, *The Historical Work of Ammianus Marcellinus* (Oxford, 1947), 129.

¹⁸ For the assimilation of outsiders, see Gilbert Dagron, "L'Empire romain d'Orient et les traditions politiques de l'Hellénisme: Le Témoignage de Thémistios," *Travaux et mémoires*, 3 (1968): 99. For other perspectives, see Max Cary, "The Frontier Policy of the Roman Emperors down to A.D. 200," *Acta classica*, 1 (1958): 131–38; and D. B. Saddington, "Roman Attitudes to the *externae gentes* of the North," *ibid.*, 4 (1961): 90–102. Christ has rightly stressed peaceful contacts across the border; however fruitful, their effect was not assimilation; "Römer und Barbaren," 282–84.

¹⁹ In the late republic, Cicero commented on conditions within Rome's sphere of domination: "there is no people that is . . . so cowed as to be subdued or so reconciled as to rejoice in our triumph and rule"; Cicero *De provinciis consularibus* 12. 31. The imperial period witnessed a wholesale transformation of these attitudes. For sensible remarks on this shift, see Jean Gaudemet, "L'Étranger dans le monde romain," *Studii clasice*, 7 (1965): 44, 46.

denizens served as necessary actors in the ritual of the imperial victory or, more practically, as a resource of manpower to be dipped into to meet imperial needs.²⁰

According to many modern scholars, the empire had an easier time keeping the barbarians at bay during the first two hundred years of its existence than it did thereafter. There is a common impulse to juxtapose the triumphant expansion of the imperial frontiers in the first century B.C. and the inroads of alien hordes in the third and fifth centuries after Christ. Although the contrast is beyond argument, it encourages the misconception that, when outward pressure ended, the reverse process of barbarian advance began, as though a coherent barbarian world had patiently awaited its chance to take the warpath against Rome. Special emphasis is invariably laid on the year 180, or thereabouts, as a turning point from easy to difficult defense. In 180, allegedly, a new and much heavier barbarian pressure began to be felt.²¹

Despite the widespread currency of 180 as the pivotal year in Rome's external relations, due caution is needed before accepting this idea, not least because it is a modern discovery. No known contemporary observer was conscious of a change.²² Nor is this surprising. Every century of Roman history had witnessed military disasters at the hands of barbarian armies. The conquests of the republican age exacted a heavy toll of lives among the conquerors, and, even during the imperial peace, barbarians sometimes annihilated large Roman armies along with their generals and forced heavy expenditures of resources for the restoration of orderly conditions. The Roman Empire never had an easy time with its neighbors; otherwise, it would scarcely have needed a burdensome professional army. Always too large in view of the limited means available to the emperor, the army was always too small for the length of the border to be defended. To us, it seems as though the epoch when Tacitus wrote was profoundly secure from external danger; yet Tacitus, by placing Rome's unimpaired destiny to world rule in dramatic contrast to the niggardliness of the goddess Fortuna, allows us to realize that, even at the end of the first century, managing the Roman Empire was a delicate balancing act.²³ Only our knowledge of barbar-

²⁰ Christ has emphasized the increased polarization occasioned in late antiquity by the *Constitutio Antoniniana*; for example, the term *barbaricum*, like its opposite *Romania*, made its initial appearance in the fourth century; "Römer und Barbaren," 279, 281-82. For the prolongation of late Roman conditions, see Kilian Lechner, "Byzanz und die Barbaren," *Saeculum*, 6 (1955): 294-96. On the changing look of the imperial victory, see Jean Gagé, "La Théologie de la victoire impériale," *Revue historique*, 171 (1933): 30-31.

²¹ M. I. Finley has made a very representative statement: "The turning point was the reign of Marcus Aurelius (who died in the year 180). The Germanic tribes in central Europe, which had been fitfully troublesome for several centuries, now began a new and much heavier pressure on the frontiers which never stopped until the western empire finally came to an end as a political organism." Finley, "Manpower and the Fall of Rome," in C. M. Cipolla, ed., *The Economic Decline of Empire* (London, 1970), 86.

²² Cassius Dio likened the accession of Marcus Aurelius's son Commodus in 180 to the passage from a golden "kingdom" to one of iron and rust, but he neither mentioned nor implied the barbarians in this connection. Moreover, Cassius Dio specified that the downfall was over by the time he wrote, a few decades later. Cassius Dio 72. 36. 4.

²³ Tacitus *Germania* 33: "Long, I pray, may the Germans persist, if not in loving us, at least in hating one another, for, although the destiny of the Empire is urging us on, fortune no longer has any better gift for us than the disunion of our foes." The translation comes mainly from Harold Mattingly's *Tacitus on Britain and Germany* (Harmondsworth, Middlesex, 1948), 128, with modifications suggested by Herbert W. Benario's "Tacitus and the Fall of the Roman Empire," *Historia*, 17 (1968): 37-50, esp. 40. Although Benario has

ian successes in late antiquity leads us to imagine that Rome's neighbors exerted less pressure at earlier times.

The Roman state continually watched the barbarians. As long as it insisted on a high level of security for the provinces, it could not take its borders for granted. Yet the barbarians were never an isolated problem. Defense of the frontiers had an assured place on the agenda of imperial priorities, but other, more pressing considerations sometimes took precedence over defense. For one thing, the government had to worry about how much in the way of resources it could squeeze out of the civilian population. The big Roman conquests had coincided with an epoch of plunder and reckless exploitation. In the sedate empire, however, the army received regular pay and the civilians were lawfully taxed. Income and expenditure had to balance. Although the empire could, and with gradually loosening restraint did, depreciate the currency, it could not use modern deficit financing. The limits upon armed expansion were aptly summed up by the third-century historian who described the exploits inaugurating the brief reign of Maximinus: "He threatened (and was determined) to defeat and subjugate the German nations as far as the ocean . . . , and his actions would have added to his reputation if he had not been much too ruthless and severe toward his associates and subjects. What profit was there in killing barbarians when greater slaughter occurred in Rome and the provinces? Or in carrying off booty captured from the enemy when he robbed his fellow countrymen of all their property?"²⁴ The prompt overthrow of Maximinus suitably rebuked his expansionist designs. In a vast and responsibly administered empire, the government could not unleash its troops on one frontier without giving thought to the question whether unendangered taxpayers were willing to foot the bill.

As the fate of Maximinus suggests, another continuing concern was internal security. Glen Bowersock has recently shown that Gibbon's main shortcoming as a historian of the empire was his persistent downplaying of domestic turbulence: "The view of almost uninterrupted peace from Augustus to Commodus depends not only on the deprecation of disturbances Gibbon mentions but on the omission of others."²⁵ After Commodus (180-92), the evidence is too obvious to be ignored. Rebellious slaves, rioting city mobs, and turbulent internal barbarians called intermittently for attention, but none of them was a persistent danger. The serious threat to the imperial government came from usurpers—usu-

rightly endorsed J. M. C. Toynbee's reading of this famous passage, the precise meaning may be better approximated by introducing the ablative absolute phrase *urgentibus imperii fatis* with "although" rather than "since." I also agree with Benario that, in the perspective of Tacitus's audience, the "fate" of Rome could not have been to fall, as we moderns are tempted to think, but rather "to rule the nations with [its] sway" (*regere imperio populos*), as Virgil put it; Virgil *Aeneid* 6. 851. For damaging invasions in the first century A.D., see Ronald Syme, "Flavian Wars and Frontiers," in S. A. Cooke *et al.*, eds., *Cambridge Ancient History*, 12 vols. (Cambridge, 1931-39), 11: 168-72.

²⁴ Herodian, *History of the Roman Empire* 7. 2-3, trans. Edward C. Echols (Berkeley and Los Angeles, 1961), 177.

²⁵ Bowersock, "Gibbon on Civil War and Rebellion in the Decline of the Roman Empire," *Daedalus*, 105 (Fall 1976): 66.

ally generals who put themselves at the head of their troops to overthrow the reigning emperor and set themselves in his place. Security from such challenges may well have been the main problem of the Roman Empire, necessarily impinging on Rome's treatment of its neighbors. For, if a choice had to be made between fighting a foreign enemy or a domestic challenger, there is no doubt which one was regarded as more dangerous. Few emperors hesitated to enroll barbarian troops to fight a usurper or to pay a border tribe for attacking him and dividing his forces. The Byzantine historian Ioannes Zonaras evoked a moment when the emperor Claudius Gothicus (A.D. 269) had to decide whether to take the field against a barbarian invasion or against a rival to his throne: "The war against [the usurper] Postumus concerns me, [but] the barbarian war [affects] the state, and its interests must be considered first."²⁶ The example is more edifying than typical. Political competitors invariably occupied a higher place on the agenda than alien enemies; barbarians were the natural allies of emperors and usurpers alike in their fratricidal struggles for power.

The Roman government had other things to worry about than just barbarians. The welfare of the provincials and the danger of usurpations were among the considerations that called as insistently for its attention as did the defense of the frontiers. There is no reason to take a gloomy view of these circumstances. The limited resources of the empire, its social tensions, ambitious generals, and aggressive neighbors ought never to be looked upon as forming a catalogue of symptoms of decline.²⁷ Such problems simply identify the complexities involved in the management of a vast and highly developed state, one whose remarkable endurance testifies to a continual power to adapt and adjust to changing conditions. In the face of this Juggernaut, the fragmented tribes beyond the Roman borders were left with little scope for pursuing their own ambitions. Indeed, the exertions and strivings of the empire determined their future together with that of the provinces. In this sense, the competing priorities among which the imperial government had to choose provided the motive force of barbarian history, the main reason why the neighbors of the Roman state escaped from their long-lasting immobility.

One last general point deserves to be mentioned—namely, the matter of invasions and migrations, what the Germans call the *Völkerwanderung* ("migration of peoples"). Many books and historical atlases contain a map purporting to show the tracks of the barbarian invaders, typically from Scandinavia to Spain and North Africa or from Central Asia to the center of Gaul. In most instances, as many as five hundred years of migration, real and supposed, are plotted on a single geographical background. By a mere glance at the arrows boldly sweeping from every direction through the Roman frontiers and provinces, the impression is gained that no empire could withstand such pressure. Moreover, the accompanying narratives tend to deploy a rhetoric of waves, tides, and floods,

²⁶ Zonaras *Epitomae historianum* 12. 26, ed. Maurice Pinder, 2 (Bonn, 1844): 604. For the first instance of cooperation with barbarians in a usurpation, see Bowersock, "Gibbon on Civil War and Rebellion," 65–66.

²⁷ On this point I part company with Bowersock's interpretation, "Gibbon on Civil War and Rebellion," 63–68.

suggesting the relentlessness of natural processes.²⁸ The maps and metaphors reflect the desire of modern historians to look upon the barbarian invasions as a purposive movement of upwardly mobile Teutons or as an irresistible force of nature—or both. Hardly anything has done more to obscure the barbarian question than the talk and images of wandering peoples tirelessly battering down the Roman frontiers and flooding into the empire.

Of course, there were invasions and migrations in the fourth, fifth, and sixth centuries. The mistake derives from lumping these various events into a generalized, self-explanatory phenomenon that in itself characterized the barbarian peoples of those and earlier centuries. The Germans, like the Celts, were sedentary agriculturalists, not nomads. Only extraordinary circumstances uprooted them. They engaged in occasional invasions throughout the imperial period and did so with little more frequency in late antiquity than before. Each incident of invasion or migration had its particular causes; people move because of specific decisions, and they often do not travel very far. Most of the prominent tribes of the invasion period, such as the Goths and the Franks, were near neighbors of the empire; there were hardly any newcomers from far away. The Huns are an exception and have to be treated as such. Demography offers another negative consideration: no single group of invading barbarians was particularly large or cohesive. Although the statistical information is meager, it would probably be wrong to estimate the size of any tribe in more than five figures, a trivial number in comparison to the millions of Roman provincials. Besides, it is important to distinguish areas like Britain and the Balkans, where immigrants permanently altered the ethnic mix, from much larger areas like North Africa, Gaul, and Spain, where barbarians remained a tiny minority precariously positioned in a Latin-speaking society. Finally, it should be remembered that the incidents of invasion were almost never one-sided. For example, the Goths crossed the Danube in 376 because the Roman government expressly allowed them to do so; the Vandals invaded North Africa in 429 because a rebellious general invited them; and the Saxons moved into Britain several decades after the Roman army had spontaneously pulled out. Once the details are considered, the idea of the invasions as a mysterious force of nature is discredited. These are ordinary historical events, in whose exposition even on maps the rules of chronology and evidence should be respected and metaphorical language strictly controlled.²⁹

In the centuries before Christ, Rome conquered as many barbarians as it saw

²⁸ For a particularly appropriate map, see Jeffrey Burton Russell, *Medieval Civilization* (New York, 1968), 95. Against maps of this sort, see Gerold Walser, "Zu den Ursachen der Reichskrise im dritten nachchristlichen Jahrhundert," *Schweizer Beiträge zur allgemeinen Geschichte*, 18–19 (1960–61): 153–54. The narrative of Lucien Musset runs the gamut from "avalanches" to "capricious ricochets"; Musset, *Les Invasions*, volume 1: *Les Vagues germaniques* (Paris, 1965), 50–74 *passim*. This language has a long past. When, toward A.D. 660, the chronicler known as Fredegar came across the statement of St. Jerome that plague had decimated the army of Marcus Aurelius, he substituted *inundatio gentium* for *pestilentia*; *Monumenta Germaniae historica* [hereafter, *MGH*], *Scriptores rerum Merovingicarum*, 2: 62 l. 31.

²⁹ For a discussion and bibliography, see my *Barbarians and Romans (A.D. 418–584)*, chap. 1. For an outstanding case study, with special attention to place names, language, archeology, and other auxiliary evidence, see B. H. Slicher van Bath, "Dutch Tribal Problems," *Speculum*, 24 (1949): 319–38.

fit to subdue; it kept the others at bay during the first two hundred years A.D. In the third century, the internal upheavals of the empire were compounded by massive invasions, but, by A.D. 305, when Diocletian retired as emperor, the intruders had been cleared out and the frontiers restored almost to their widest dimensions. The climactic encounter of the empire with its neighbors occurred thereafter, beginning in the 370s and continuing through the sixth century. Before turning to this critical phase, it may be appropriate to sum up the general considerations that have occupied us up to now.

First, the term "barbarian" is unobjectionable as long as we recognize that it expresses a Greco-Roman point of view. In the language of the empire, its neighbors sometimes formed a collectivity. To themselves, however, those whom the Romans embraced in one word were diverse and disunited. Since they had no collective mind or collective aspirations, it is incorrect to evoke a single "barbarian side" and to believe that a history can be written from that perspective.³⁰ In order to narrate the invasions from a non-Roman standpoint, as many different circumstances and motives would have to be portrayed as there were barbarian peoples. The lack of adequate information prevents us from even beginning this task.

Next, there was nothing new or unexpected about the barbarian problem. Relations with foreigners, peaceful or hostile, had a constant place on the imperial agenda, but only as one item among others. The government had to choose among competing priorities in determining the actions to adopt in any given case. Year by year, it had to decide how much defense the taxpayers could afford or whether repulsing a barbarian incursion took precedence over eliminating a challenger to the throne. The ability of barbarians to assert themselves hinged far less upon their strength and wishes than upon the response that the Roman government could make to them in the short and long run.

Last, there is no truth to the idea that the barbarians were carried forward in a relentless migration from the wastes of the Baltic to the Mediterranean sun. Although deeply rooted in the tradition of European historical writing, this tale is just as mythical as more ancient origin stories, such as the one that made Aeneas, a refugee from Troy, the forefather of the Roman people. In its twentieth-century guise, the myth of Germanic migrations has drawn its vigor from a patriotic desire "to roll away the enormous burden of biblical-classical convention in historical conceptualization and to find an independent point of departure for German history outside the *orbis universus*."³¹ To perpetuate this

³⁰ To the contrary, Gerald Bonner: "Future historians will have to . . . try to build up a comprehensive picture of the German invasions from both sides"; Bonner, Review of Pierre Courcelle, *Histoire littéraire des grandes invasions germaniques* (3d ed., Paris, 1964), in the *Journal of Roman Studies*, 56 (1966): 247. It is impossible to build up a "side" on so narrow a basis as grave goods; archaeological evidence from *barbaricum* is necessarily contaminated if conflated with literary testimony, since the latter is of imperial origin and, *inter alia*, strives for schematic simplicity.

³¹ Hermann Aubin, "Zur Frage der historischen Kontinuität im Allgemein," in his *Von Altertum zum Mittelalter* (Munich, 1949), 70. For "the German 'romantic fallacy'" that accompanied the incomparable contribution of German scholars to the study of medieval history, see, in brief, M. D. Knowles, "Some Trends in Scholarship, 1868-1968, in the Field of Medieval History," *Transactions of the Royal Historical Society*, 5th ser., 19 (1969): 140-43.

myth merely prolongs the wishful thinking that has nurtured it thus far. The migrations that matter in the history of Rome and the barbarians do not embody mysterious forces. They were the expressions of human decisions, some known to us and others not, and it is doubtful that they were more momentous than armed conflicts in determining the outcome of events.

WHAT COMPELS OUR ATTENTION in the tale of Rome and the barbarians is the massive, unmistakable, and comparatively rapid replacement of Roman by alien rule in the western provinces of the fifth century. The temptation to emphasize earlier developments, strong as it is, should be restrained. We have seen what must be made of the contrast between easy and difficult defense or between light and heavy pressure. Our ability to measure shifts in ratios of force along the borders is notably defective; besides, we have little assurance that they mattered.³² Scholars have been right to emphasize that many more officers and troops from among distant tribes were recruited into the Roman army in the 300s than before. Yet the documented tendency of recruiting officers to shift their activities in every century from developed districts to more backward ones prevents us from imagining that the practice of late antiquity was a radical novelty.³³ Theodor Mommsen and Alfons Dopsch, among others, liked to think that the invasions were prepared by a slow "Germanization" of the empire through the infiltration of alien peasants and soldiers.³⁴ Such a view presupposes the existence of a uniform Germanic or at least barbarian identity. The certainty of its absence implies that the infiltrators were more likely to acquire coatings of Greco-Roman culture than to impress their variegated ethnicities upon their adopted home. Identifying the cultural distinctiveness of barbarians in the sixth century is hard enough without supposing that the empire in the third and fourth centuries was affected on more than a small, local scale by its numerous but scattered alien subjects. Although the "fall of Rome" was, of course, neither sudden nor catastrophic, we are well advised to concentrate our attention on an unambiguous break with tradition—namely, the treaties allowing barbarians to settle with autonomy inside the provinces, the context in which these treaties were made, and, most of all, the progressive abandonments of territory they occasioned. After this final change is summarized, four questions that seem crucial in an analysis of the events will be examined at greater length.

The last emperor of the West Roman line was overthrown in A.D. 476 by Odoacer, a barbarian general in his employ. Odoacer undertook thereafter to rule Italy and adjacent lands as the delegate of the Roman emperor in Constantinople. Since the opening of the fifth century, military control of many

³² On the latter point, see my discussion of the Saxons in Britain, pages 294–95, below.

³³ There is a large bibliography on this subject. For recent work, see K. F. Stroheker, *Germanentum und Spätantike* (Zurich, 1965), 9–29, 30–53; and Manfred Waas, *Germanen im römischen Dienst im 4. Jahrhundert n.C.* (Bonn, 1965). On the practices of recruiters, see Andreas Alföldi, "La Grande Crise du monde romain au III^e siècle," *Antiquité classique*, 7 (1938): 10–11.

³⁴ Dopsch, *Economic and Social Foundations of European Civilization*, abr. trans. M. G. Beard and Nadine Marshall (London, 1937), 49, 52–53; Dopsch cited Mommsen's opinion.

western regions had been gained, more often than not with official Roman sanction, by barbarian peoples: Vandals in North Africa, Visigoths in Spain and Gaul, Burgundians in southeastern Gaul, and a few others elsewhere. In 476, these dominations became independent kingdoms. Britain had been evacuated about 407 and transferred to the control of its Romano-British inhabitants, who, in the 470s, were still fighting off attacks and immigration from several directions. The Roman state, in its Constantinopolitan incarnation, had not wholly given up the western provinces. Under its auspices, toward 490, a strong force of Ostrogoths seized Italy from the "tyrant" responsible for the coup d'état of 476. Constantinople maintained active diplomatic relations with the western kingdoms and did not shrink from intervening in their internal affairs. From 533 to 555, expeditionary armies under its direct command wrested North Africa from the Vandals, Italy from the Ostrogoths, and even a part of Spain from Visigothic rule.³⁵ After the loss of much of this conquest, however, notably to the Lombard invasion of Italy in 568, there opened a period of relative stability interrupted only by the unrelated (for the purposes of this essay) expansion of Islam in the seventh century.

As is well known, Roman surrenders of territory in the fifth century were preceded by two successive barbarian incursions. The first of them was not an invasion but, rather, a rebellion of Goths who had been peacefully admitted across the Danube in 376. This rebellion was punctuated in 378 by a devastating defeat of Roman arms at Adrianople in Thrace, where the emperor Valens and many others lost their lives. The other incursion occurred on New Year's Eve, 406, and saw a variegated horde—containing Vandals, Alans, and Sueves—breaking across the Rhine, rushing destructively through Gaul, and, in 409, taking control of a large part of Spain.³⁶ Though dramatic and hideous, these incidents were hardly unprecedented in Roman history. Some twenty years passed between the first of them and the next, and even later in the fifth century there were long breathing spaces during which recovery among the Romans was not only possible but actually took place in various forms.³⁷

However risky it is to expect uniformity of policy across long periods of time, one may hardly avoid contrasting the circumstances of 376–82 and 406–18 to those of the third century: why did the emperors of the late fourth century not respond to massive barbarian incursions in the same way that their predecessors had one hundred and fifty years before? From the 250s to the end of the third century, the empire experienced many severe attacks at different points on the borders. An attentive examination of these incidents suggests that, in the short

³⁵ For the events noted in this paragraph, see the standard modern narratives: J. B. Bury, *History of the Later Roman Empire*, 2 vols. (reprint ed., New York, 1958); Ernst Stein, *Histoire du Bas-Empire*, trans. J. R. Palanque, 2 vols. (Paris, 1949–59); and A. H. M. Jones, *The Later Roman Empire*, 170–317.

³⁶ One more might be added: the rush on Italy by a horde under the leadership of Radagaisus in 405. What sets this incursion apart from the other two is that the attackers were completely annihilated the next year.

³⁷ The spirit of renewal in the early fifth century (documented, for example, by the poem of Rutilius Namatianus) has been commented on many times. The idea is captured by the title of Arturo Solari's work, *Il Rinnovamenti dell'impero romano*, 2 vols. (Genoa, 1938–41). Also see my remarks (and references) concerning the middle of the century in "The Date and Purpose of Vegetius' *De re militari*," *Traditio*, 23 (1977): 87–88, nn. 105–06.

run, they were considerably more damaging than the invasions experienced from the 370s on. Nevertheless, all of the third-century intruders were annihilated or expelled, and the frontiers were restored. Some sections of the empire were evacuated, notably the province of Dacia north of the Danube; on the border with Persia, new territory was annexed. The whole process was long and very costly, but the job was done.³⁸ Successive emperors and their advisors evidently took the view that strenuous armed defense was the correct response to barbarian attacks. At whatever sacrifice, the aliens had to be driven out, and they were.

Although faced with more isolated attacks at longer intervals, the Roman government after 378 departed from the military solutions of the third century. Armed force was used only to contain the damage, to pin the Goths, Vandals, or whomever into positions where they became disposed to negotiate and make a treaty. The main instrument of the late Roman government for ending barbarian aggression was to grant the offending tribe an area of settlement within imperial territory. First made in 382, such grants became a recurrent pattern. Their result by the 450s, as we have seen, was a set of autonomous Gothic, Vandal, and Burgundian districts in the western provinces, an apparent dismemberment of the empire in the West.³⁹

The pertinent question why western districts were singled out for surrender will be considered shortly. For the moment, it suffices to ask why did the later emperors behave so differently from their third-century predecessors. The currently accepted answer seems to be that they had no choice: either the Goths were too strong or sufficient resources were not available to permit a strenuous Roman counterattack.⁴⁰ No one, however, has been able to this day to mount a convincing proof suggesting that these answers are correct. The strength of the Goths and other relevant barbarians does not, in isolation, bear examination. As for the resources of the empire, every modern attempt to argue depopulation, agricultural disaster, or calamitous overtaxation has failed when subjected to critical scrutiny.⁴¹ Nevertheless, we continue to be told that the inability of the empire to master "disorganized peoples who did not wish to be its enemies and of whom none numbered more than 100,000" can only be explained by its having been thoroughly impoverished in manpower, supplies, and money.⁴²

³⁸ A. Alföldi, "The Invasions of Peoples from the Rhine to the Black Sea" and "The Crisis of the Empire (A.D. 249–270)," and H. Mattingly, "The Imperial Recovery," in *Cambridge Ancient History*, 12: chaps. 5–6, 9; Fergus Millar, *The Roman Empire and Its Neighbours* (London, 1967), 239–48; and Michael Grant, *The Climax of Rome* (Boston, 1968), 13–65.

³⁹ For the mechanics of these territorial awards, see my *Barbarians and Romans (A.D. 418–584)*, chaps. 2–5.

⁴⁰ Matthews's phrase—"Gothic pressure on the frontiers was in the long run irresistible"—is particularly representative; see note 2, page 275, above.

⁴¹ On barbarian strength, see P. A. Brunt, "Reflections on British and Roman Imperialism," *Comparative Studies in Society and History*, 7 (1964–65): 276. For major rebuttals of Roman internal weaknesses, see M. I. Finley, Review of A. E. R. Boak, *Manpower Shortage and the Fall of the Roman Empire in the West* (Ann Arbor, 1955), in the *Journal of Roman Studies*, 48 (1958): 156–64; and C. R. Whittaker, "Agri deserti," in M. I. Finley, ed., *Studies in Roman Property* (Cambridge, 1976), 137–65. Finley's review argues against the thesis of depopulation. Although inclined to minimize internal problems, A. H. M. Jones once argued the case for overtaxation; see his "Over-Taxation and the Decline of the Roman Empire," *Antiquity*, 33 (1959): 39–43.

⁴² Haller and Dannenbauer, *Der Eintritt der Germanen in die Geschichte*, 61.

The danger here is not so much that the argument circularly infers cause from effect but that the notion of impoverishment, unless it is meant as a vague metaphor, focuses upon the passive condition of imperial wealth rather than upon the desire and determination of the state to mobilize such wealth as there was. The resources of the Roman world in any age are bound to look paltry in modern eyes, but that impression is irrelevant. In view of the domestic and external shocks suffered by the third-century empire, especially in the later 200s, its aggregate resources could hardly have exceeded those available for mobilization after Adrianople, yet they had sufficed for clearing the provinces of barbarian intruders. For lack of convincing proof to the contrary, we may only assume that the wherewithal for war was at least adequate in the fourth and early fifth centuries. Growing knowledge of the later empire tends to confirm this view. Not even the evocation of corrupt bureaucrats and greedy noblemen, often encountered in modern scholarship, quite suffices to explain why strenuous defensive exertions were avoided.⁴³ Any profiteer from the regime risked—and would endure—greater losses than small farmers from the dismemberment of the empire.

There is much, to be sure, that we still do not know about the economic history of the fourth century. A. H. M. Jones summed up a few important and well-established facts about its course: in the late 200s and early 300s, prolonged inflation imposed the need for radical remedies upon “the imperial government, whose fiscal system was so rigid that the tribute . . . could not be raised to counterbalance the decreasing value of the currency. The state was thus forced back onto an economy in kind, paying its soldiers and civil servants mainly in food and clothing, and making levies in kind to supply its needs. . . . By the early fifth century [however] the Empire was on a gold economy, nearly all taxes, salaries and rents being paid in *solidi*, and all important economic transactions being conducted in *solidi*.”⁴⁴ Cheerful conclusions are usually drawn from this return to the economic normality of major transactions in sound currency. But is that favorable assessment altogether adequate? The Roman state that had requisitioned goods and issued rations to its employees had also been the one that strenuously cleared the provinces of barbarians, whereas the state of the early 400s, which collected and dispensed gold *solidi*, took the soft option of quartering autonomous aliens in its lands. The gradual reconversion of public finance from levies in kind to taxes in gold quite possibly built such rigidities into the economic fabric of the empire that the government was denied the choice of extraordinary defense. Nevertheless, an economic determinism of even this limited kind has yet to be confirmed. The terrain is promising but its harvest is still in doubt.

⁴³ George R. Monks, “The Administration of the Privy Purse: An Inquiry into Official Corruption and the Fall of the Roman Empire,” *Speculum*, 32 (1957): 748–79. On selfish magnates, after the work of Johannes Sundwall and Ernst Stein, see M. T. W. Arnheim, *The Senatorial Aristocracy in the Later Roman Empire* (Oxford, 1972).

⁴⁴ Jones, in *Third International Conference of Economic History, Munich, 1965* (Paris, 1969), 104. Most general accounts of the economic decline of the empire seem still to derive, directly or indirectly, from an essay by Max Weber that was superb when originally published in 1896; see his “The Social Causes of the Decay of Ancient Civilization,” trans. C. Mackauer in *Journal of General Education*, 5 (1950–51): 75–88.

A subtler, and more certain, connection between economic conditions and responses to barbarians is provided by Gilbert Dagron's study of the orator Themistius, a pagan Constantinopolitan whose public career extended from the 350s, under a son of Constantine, to Theodosius I in the 380s.⁴⁵ Themistius was not normally haunted by thoughts of a barbarian menace or of the need for greater exertions against mounting pressure. Even when he had the frontiers in view, he was concerned above all with cutting state expenses:

The emperor must understand that the true king, like Apollo, bears the lyre as well as the bow. . . . Like a shepherd, he must pay even more attention to the health of his flock than to such external dangers as wolves and wild beasts.

By neglecting internal health for external glory, Cyrus, Darius, and Alexander showed that "they were generals, not kings. The praise of a general is to have defeated his enemies; that of a king is the happiness of his subjects."

"Booty and prisoners profit only those who bear arms . . . ; the true victory is that which will allow us to be equally unconcerned about the Scythians [= Goths] and about the tax collectors. . . . I shall take account of the plunder taken from the Scyths only when no one will any longer plunder me [for taxes]. . . . To a wretch it hardly matters whether his misery results from the act of a Scythian or from that of a Roman."

"In sum, a unanimous commitment can be aroused only by what is undertaken for the benefit of all. The point is not to recover Mesopotamia [from the Persians] or to bring back to reason the Scythians beyond [the Danube] or to rebuild the cities ruined by the Germans: even if we succeeded in doing so, the only ones to notice would be the Syrians, Thracians, and Gauls, and the victory in each case would belong only to the neighboring territory. But if a moderate fiscal policy were conducted, it would be for the common benefit of all those inhabiting the world."⁴⁶

The implied preference for inactivity vis-à-vis barbarians is of little moment in this case, for Themistius spoke when the frontiers were still secure. Even in more aggressive ages, peace had been at least as common a counsel as war for relations across the borders. Neither was it unorthodox for someone to maintain that the triumphs of Roman armies were bought by the tears of the taxpayers.⁴⁷ What does sound novel is that Themistius sharply opposed the interests of threatened provinces to those of the total population. Fighting against barbarians, he alleged, was advantageous only to the provincials of Syria, Thrace, and Gaul, whereas the general interest—that of Romans as a whole—consisted of peace and tax cuts. Such a sentiment would not surprise if it came from an ordinary resident of a safe district near the Mediterranean; the empire was too large and individual horizons too narrow to permit a widespread sense of inter-provincial solidarity. But Themistius belonged to the Establishment. He was a learned philosopher, a senator of Constantinople, broadly representative of those close to the government who spoke for civilian interests and peaceful solutions. When a man of his stamp actively denied that the security of a part of the em-

⁴⁵ Dagron, "Le Témoignage de Thémistios," 1–203.

⁴⁶ Themistius *Oratio* 15 (A.D. 381), 10 (A.D. 370), and 8 (A.D. 368), in Dagron, "Le Témoignage de Thémistios," 101–04, nn. 125, 129. The passages not set off by quotation marks are translated from Dagron's paraphrases.

⁴⁷ On peace, note, for example, Hadrian's reversal of Trajan's expansionism and Commodus's of Marcus Aurelius's. For the tears of taxpayers, see Herodian, *History of the Roman Empire* 7.2 (page 282, above); and, for Marcus Aurelius's reply to the army's demand for higher pay, see Cassius Dio 71. 3. 3.

pire was a good cause for collective sacrifice, his position raises the question whether the unified system for defending the empire had any future at all.

Most of the remarks of Themistius that I have quoted date from 368. His reasoning would have been even more pertinent ten years later, as an argument against a military solution to the chaos following the disaster of Adrianople: the cost of expelling the Goths could not be justified since the benefit would accrue only to a small segment of the population. By implication, any fraction of the empire, such as distant Britain, whose retention called for extraordinary expense was worth abandoning for the simple reason that it was merely a fraction. It is little wonder, then, that, when in 382 the emperor Theodosius I made a peace with the Goths that accorded them autonomous status within the empire, Themistius immediately affirmed that this was not merely the correct but indeed the only possible course of action.⁴⁸ If the primary obligation of a true king was to shield the bulk of his subjects from sacrifices, then Themistius was right: even a major and unprecedented concession to barbarians should be welcomed. The critical element, however, was neither Gothic strength nor deficient Roman means; it was a scale of imperial priorities in which the repose of the many had an absolute preference over the safety of a few.

Where barbarians are concerned, even in the fourth century, the variable that merits attention lies on the Roman side of the frontiers. The oddity of the age was the government's resort to accommodation in preference to prolonged military action. Was the state forced to avoid the strenuous course of eliminating foreign intruders, or did its concessions express a conscious choice? Although the possibilities of a generalized collapse of Roman resources and of class selfishness are unpromising, more subtle forms of economic determinism could have played a part in the outcome. Even if they did, it does not follow that the empire had no alternative. Then, as in the past, its desire to maintain the integrity of the territory had to be balanced against competing considerations. Unless the Roman state can be shown to have somehow lost all freedom of maneuver, we are better advised to assume that it considered peace with barbarians to be positively preferable to extraordinary costs and military exertions. The late Romans surely did not want privileged aliens in their midst, but there were other things that they wanted even less.

An additional reason for this conclusion will become apparent in examining a second question, one that has often been asked before. It arises from the observation—rightly stressed in the literature—that the barbarian invasions had a more pronounced effect on the western parts of the Roman Empire than on the eastern provinces. As everyone knows, an imperial regime descending in unbroken succession from Augustus and Constantine survived the crises of the fifth century and prolonged its existence for another thousand years as the Byzantine Empire. The Roman state that ended in 476 was the empire of the west.⁴⁹ But why only there?

⁴⁸ Dagron, "Le Témoignage de Thémistios," 103–06.

⁴⁹ For an important statement of this theme, see Norman Baynes, "The Decline of Roman Power in Western Europe and Its Modern Explanations," in his *Byzantine Studies and Other Essays* (London, 1955), 83–96.

Several explanations for this regional difference in fates have an honored place in modern narratives. Allegedly, the Roman west had longer frontiers, more ferocious enemies, a smaller population, an underdeveloped and less urbanized economy, and so forth. Not all of these statements are false, but the answers they supply run the risk of being as hasty as the hypothesis of empire-wide impoverishment. The barbarians who threatened the western provinces were, in fact, less dangerous than the Persians in the east or the Goths on the lower Danube, and the west was rich in military manpower. Not only did it regularly send reinforcements eastward until as late as the 360s, but these troops were also acknowledged to have superior discipline and fighting qualities. Although the west lacked imported luxuries, it was at least as well provided as the east with essential military supplies like iron and wheat.⁵⁰ When conditions like these are placed in an accurate perspective, only one glaring and potentially dangerous contrast between the two parts of the empire can be identified: right down to 425, the west was a nursery of pretenders to the throne, and the armies of Britain and Gaul, which repeatedly backed these usurpers, were the outstanding threat to the security of the emperors. In this respect as well, a consideration of domestic priorities offers a better explanation for the activities of the barbarians in late antiquity than their own aspirations and strivings.

A closer look at the problem of internal security should begin with the observation that the "Western Empire" that was overthrown in 476 acquired its distinctive regional form only some eighty years before its end. The permanently junior partnership that the western emperor occupied vis-à-vis his Constantinopolitan colleague was prefigured by the effaced role of Valentinian II from 388 to his death and fully realized in the reign of Honorius, the younger son of Theodosius I, from 395 to 423. Although Honorius himself became senior emperor at the death of Arcadius in 408, as did Valentinian III at that of Theodosius II in 450, neither one was able or allowed to claim the prerogatives of seniority.⁵¹ Constantinople had insulated itself from political intervention by the west. Barbarian hostility had not been the cause for this precaution.

The late emergence of a distinctly western emperorship—a separate dynastic branch that buried its members in a mausoleum of its own—becomes understandable when one realizes that Constantine's establishment of a capital at Byzantium in 330 had not been meant to divide the empire into two parts. At its origin, Constantinople had been a new Rome, ruling a united realm as Rome had once done. The new Rome, like the old, was located in Europe, and it was designed to be the seat of one dynasty.⁵² This Constantinian settlement, super-

Also note H. I. Marrou, *Saint Augustin et la fin de la culture antique* (4th ed., Paris, 1958), 701 n. 2: "Le véritable problème porte sur ce point: pourquoi l'Occident a-t-il succombé, pourquoi l'Orient a-t-il résisté aux coups des barbares, qui ne lui ont pas été épargnés?"

⁵⁰ The comparatively greater, and dual, threat to the Danube and Syrian frontiers is documented by the shift eastward of the armies and the emperors, a pronounced tendency since the end of the first century A.D. For a vigorous argument against the supposed economic weakness of the west, see Ferdinand Lot, *Nouvelles recherches sur l'impôt foncier et la capitation personnelle sous le Bas-Empire* (Paris, 1955), 159–79.

⁵¹ In addition to the standard works cited in note 35, page 287, above, see J. R. Palanque, "Collégialité et partages dans l'Empire romain aux IV^e et V^e siècles," *Revue des études anciennes*, 46 (1944): 47–64, 280–98.

⁵² Gilbert Dagron, *Naissance d'une capitale: Constantinople et ses institutions de 330 à 451* (Paris, 1974), 19–76.

seeding the multiple nondynastic emperors and far-flung headquarters of the Tetrarchic regime, had brought promise of lasting civil peace. It enjoyed widespread support, and not only in the east. A comparison with the record of military usurpations from 235 to the downfall of Licinius in 324 shows that Constantine achieved for the age following him a startling improvement in internal stability. Not surprisingly, however, he did not attain perfection. The distant west, with its strong armies guarding Britain and the Rhine frontier, remained a point of instability, a springboard for generals with the ambition to make a grab for power. From there, in fact, Constantine himself had launched his astonishing career.

Between 324 and 475, the environs of Constantinople witnessed only one usurpation: the serious but brief rising of Procopius in 365. Yet the government was repeatedly challenged. The battle fought at Mursa in A.D. 351 between Constantius II and the western usurper Magnentius was, according to the historian Eutropius, the most tragic clash experienced by the Roman world since Caesar met Pompey at Pharsalia (48 B.C.). Ten years later, Julian verged on retracing the career of his uncle Constantine, as well as that of Magnentius, when he moved the armies of the west against the Constantius II; only the latter's demise from natural causes averted another bloodletting. The events following Julian's death and that of Valentinian I in 375 show that the political unreliability of the *exercitus Gallicanus* was a fact for statesmen to reckon with, even apart from outright rebellion: its doubtful loyalty was a major reason for Jovian's excessive concessions to Persia in 363 and the only one for the proclamation of the infant Valentinian II in 375. The latter's father, Valentinian I, had taken extraordinary precautions to win the support of the western troops for the dynasty he hoped to found. Rather than establish himself in Constantinople, where his body was in time brought for burial, he took personal charge of the Rhine frontier, spent ten years of his reign in military action against western barbarians, and went to the lengths of marrying the widow of Magnentius while his first wife was still alive. Even so, Valentinian failed in his dynastic objective: first one and then the second of his sons fell victim to a pretender. Their avenger, Theodosius I, found use for the Goths after Adrianople by recruiting them and many other barbarians to suppress the two usurpers who, within ten years, won the backing of the western armies. Even at Theodosius's death in 395, the story of western usurpations is hardly finished. From 406 to 413 and again in 423–25 new challenges arose against a legitimacy rooted in Constantinople. They were all put down.⁵³

A haunting echo of these conflicts reaches us in the accounts of the historians

⁵³ On Procopius's usurpation, see Ammianus Marcellinus 25.9.13, 26.5.8–26.10. On Pharsalia and Mursa, see Eutropius *Breviarium* 6.21, 10.12.1; in each case, Eutropius noted the wasting of forces that, combined, were capable of conquering the whole world. On Julian's usurpation, see Ammianus 20.40–21.15. On the *exercitus Gallicanus*, see *ibid.*, 25.5, 25.8.8–12, 25.9.8, 25.10.6–10, 26.1.3–6, 26.4.1, 27.6, 30.10. On Valentinian's inability to leave Gaul and fight Procopius, see *ibid.*, 26.5.1–7, 11–14; and, on the punishment of Valentinian's father for dealings with Magnentius, see *ibid.*, 30.7.3. On Valentinian's burial place, see *ibid.*, 30.10.1. About his curious marriage, see Socrates *Historia ecclesiastica* 4.31; according to Ammianus, Valentinian was not lustful; see Ammianus 30.9.2. On Gratian and Valentinian II, see Stein, *Histoire du Bas-Empire*, 1: 200–17; and, on usurpations after 395, see Bury, *History of the Later Roman Empire*, 1: 185–202, 221–25.

Gildas, Bede, and Geoffrey of Monmouth about rulers who stripped Britain of its troops and crossed to the Continent, leaving the island to its own devices while they engaged in adventure against the power of Rome.⁵⁴ Though too simple, this tale is not without truth for the course of events on all of the western frontiers in late antiquity. How much wealth in men as well as goods was lost by the Roman west in the unsuccessful usurpations of the fourth and early fifth centuries is a matter for conjecture; none of them, so far as we can tell, undermined the productive capacities of the provinces. Even in the fifth century, Sidonius Apollinaris and his aristocratic friends enjoyed a life of abundance, and the number of churches was continually increased by new construction.⁵⁵ The weakening of defenses occasioned by civil war finds a more likely cause in the disposition of the Constantinopolitan victors. What successor of Valentinian I, even if otherwise unoccupied, would have been tempted to curry favor with the *exercitus Gallicanus*? More to the point, western defenses against barbarian incursions could have been restored to normal standards of security only if the triumphant government had made determined efforts after 388 and 394 to rebuild and discipline the armies that had followed the latest usurpers. Yet the lesson of recent events plainly counseled against such a course. Weakness in the west was the condition of security for the imperial throne, and the attachment of generals to the dynasty was a more important consideration than their military skill. In 394 and 395, therefore, the frontier forces were patched up any which way, a younger son was established as ruler in Milan, and an imperial relative by marriage became generalissimo of the west, with headquarters in Italy rather than on an exposed border.⁵⁶ The security thus attained subordinated external to domestic dangers. In view of recent history, it was a wise choice.

About one hundred and fifty years later, a historian writing at Constantinople had so dim a perception of the situation of the west as the fourth century closed that he imagined that Gaul and Spain were "almost lost" in the opening years of Honorius's reign, after 395, rather than only after the breakthrough of 406. The idea he shared with his contemporaries was that the empire had been diminished by official "negligence."⁵⁷ Such accounts are less wrong than the precise record of Roman control of the west makes them seem. The slow unfolding of fifth-century events testifies to the lack of assertive barbarians capable of making rapid headway against even feeble resistance. To judge by the nearly two hundred years that the Saxons took to gain dominance in a Britain whose garrison, once evacuated, was never restored, only sluggish change

⁵⁴ Gildas *De excidio et conquestu Britanniae* 13–14; Bede *Historia ecclesiastica gentis Anglorum* 1. 11–12; and Geoffrey of Monmouth *Historia regum Britanniae* 3. 7–10 (Belinus and Brennius), 5. 12–6. 1 (Maximianus), 9. 15–10. 13 (Arthur). Only the second of these expeditions is portrayed as damaging to Britain.

⁵⁵ On the society of Sidonius, see Sir Samuel Dill, *Roman Society in the Last Century of the Western Empire* (2d ed., London, 1906), 187–223. For a survey of church construction, see May Vieillard-Troiekouroff, *Les Monuments religieux de la Gaule d'après les oeuvres de Grégoire de Tours* (Paris, 1976).

⁵⁶ See my Review of J. F. Matthews, *Western Aristocracies and Imperial Court, A.D. 364–425* (London, 1975), in the *English Historical Review*, 91 (1976): 353.

⁵⁷ On the loss of Gaul and Spain, see Jordanes *Getica* 154. Jordanes placed the Vandal Gaiseric too early by a generation; compare this passage with *ibid.*, 141, 145. About "negligence," see Jordanes *Romana* 332; and my "Zosimus, the First Historian of Rome's Fall," *AHR*, 76 (1971): 432, n. 88.

could be expected from a confrontation of barbarians and ordinary Romans.⁵⁸ The more rapid pace of events on the Continent, where the government exercised a continual solicitude, illustrates in itself the active role of imperial policy in transferring military control to Gothic, Frankish, and Vandal chieftains and their followers. There was logic in these surrenders, though not one of deficient material resources or even, necessarily, of a deficient will to impose sacrifices upon the population. The same imperative of internal security that argued in the 390s against the rebuilding of the armies of Gaul and Britain positively favored, in the next century, the quartering of barbarian forces among the provincials of the west. A weak offspring of the ruling house, or an equivalent representative, was now a permanent resident of Italy. The regime was safer with alien defenders than with Roman armies, for, however objectionable the former might be in other respects, nationality excluded them from having designs on the imperial throne. Besides, settled barbarian troops placed no more of a financial burden on the taxpayers than the armies had. Though headed by royal families, the Goths and others had no clear notions of sovereignty and independence; their status amidst a numerous population of Latin speakers depended in part on the maintenance of satisfactory relations with the imperial power; Roman law was not abrogated in the territories they occupied; and the property of even rich natives was normally respected.⁵⁹ The aristocracies and clergy of the affected regions retained positions of great dignity and eminence.⁶⁰ All in all, the losses endured by the Roman state might merely be transitory, whereas the immediate gains to it were beyond doubt. For the long span from 425 until Heraclius sailed from Carthage to overthrow Phocas in 610, Constantinople never had to worry about a challenger out of the west.

The third question is suggested by the indecisiveness and ambiguity of fifth-century events. As one decade succeeded the other, the advantages secured from installing barbarians in the western provinces were not clearly outweighed by the losses and dangers that their presence occasioned. No westerner at the opening of the sixth century wished to repudiate the overriding dominion of the emperor of the new Rome. The more or less orderly garrisoning of Gaul, Spain, Africa, and Italy by alien troops gives us no compelling reason to speak of a "barbarian west." Not even the evacuation of Britain or the overthrow of the too aptly named Romulus Augustulus was conclusive. The west was still full of "Romans" speaking Latin and worshipping as Catholics in a church whose temporal leadership was recognized to be divinely vested in the emperor. The sprinkling of resident Goths, Burgundians, and other certifiable barbarians mostly professed a form of Christianity that the Catholics considered repugnant and heretical, but there was no lack of pressure on them to abandon their Arianism

⁵⁸ For the course of events in Britain, see Peter H. Sawyer, *From Roman Britain to Norman England* (New York, 1978), 76–91. I have reservations about the alleged effects of plague after 549; *ibid.*, 18, 85–86. Something would have to be subtracted from my count of two hundred years if, as John H. Ward has maintained, the evacuation of Britain in 407 was not final; see his "The *Notitia Dignitatum*," *Latomus*, 33 (1974): 430–31.

⁵⁹ On these matters, see my *Barbarians and Romans (A.D. 418–584)*.

⁶⁰ For documentation (and a rich bibliography) of an important dimension of aristocratic pre-eminence, see Martin Heinzelmann, *Bischofsherrschaft in Gallien* (Munich, 1976).

and join the majority they lived among.⁶¹ Religion aside, the faces of resident barbarians are revealed in Latin law codes, state papers, and panegyrics—artifacts whose Roman character is obvious.⁶² Their prominent leaders in the early 500s were Theodoric the Ostrogoth, who gained the reputation of ruling Italy with all of the talent and benevolence of a good emperor, and the Frank, Clovis, who turned directly from a pagan into a Catholic. These conditions must make us wonder. How did it happen that the west “went barbarian”? Armed conquest by aliens could surely have little more to do with the matter. Yet the Catholic Latins, ancestors of Romance Europe, eventually found contentment in adopting the tribal names of their rulers and thus becoming honorary Goths, Burgundians, Franks, and, in due time, even Lombards. Is this because the new dynasties rallied their admiration or because the imperial masters at Constantinople cast all of the westerners adrift to find whatever new moorings they could?

When the strength of Catholicism, Latinity, and imperial loyalties in the west is kept in mind, the main oddity of the sixth century is that the emperor Justinian undertook western conquests. Although the paltry barbarian rulers of Africa and Italy posed no threat to him, he seemed to think that they needed to be subdued by force of arms. We risk missing an important point if we take Justinian’s “reconquest” at face value and assume that it was the correct response, at long last, to barbarian domination of Roman territory. The passage of time makes a difference. That there should have been no prolonged military response after 378 and 406–07 is a fact that merits astonishment; with martial emperors like Julian and Valentinian I only shortly dead, the absence of a Roman counterattack calls for explanation. But Justinian belonged to an age when the example of aggressive emperors like Trajan, or the Illyrians of the late 200s, had become entirely anachronistic. For him to undertake western expeditions was to depart sharply from the vision of the Theodosian dynasty that had reconciled the quartering of barbarian federates with a positive enhancement of imperial security from the recurrence of civil war. Although it is wrong, as we shall see, to single out Justinian as an innovator, his conquests pose the genuine problem of how Constantinople came to conceive of the west as “lost” and to regard the barbarians who controlled its provinces as amenable to little reason other than that of armed force.

⁶¹ The early “authorized version” of Gothic Arianism was that the heresy was foisted on them by the wicked emperor Valens, whom God duly punished; this is the story told by Orosius and the trio of Greek church historians. Salvian’s attitude toward barbarian Arianism is not hostile. The Burgundians slipped from orthodoxy to Arianism and back to orthodoxy; the Sueves in Spain also vacillated. The image of nasty barbarian Arians is most firmly connected to the Vandals (Victor Vitensis, 480s), but their ruling house was divided in adherence prior to Justinian’s attack, owing to a marriage tie with the imperial house. See E. A. Thompson, “Christianity and the Northern Barbarians,” in Arnaldo Momigliano, ed., *The Conflict between Paganism and Christianity in the Fourth Century* (Oxford, 1963), 56–78. He has taken comparatively little account of Roman opinion.

⁶² For the Visigothic code of Euric (ca. 475) and the Burgundian code of Gundobad (ca. 500), see the challenging observations of Patrick Wormald’s “*Lex Scripta and Verbum Regis: Legislation and German Kingship*,” in P. H. Sawyer and I. N. Wood, eds., *Early Medieval Kingship* (Leeds, 1977), 105–38. For state papers, see the *Variae epistolae* of Cassiodorus, from the court of the Ostrogoths in Italy. For panegyrics of Theodoric and Euric, see Sidonius Apollinaris *Epistolae* 1. 2, 8. 9. 5; and, for one of Theodoric the Ostrogoth, see Ennodius of Pavia *Opera*, ed. W. Hartel, 261–86. Even the grave goods of Childeric (482) express Constantinopolitan, rather than “Germanic,” tastes; Peter Lasko, *The Kingdom of the Franks* (New York, 1971), 26.

The problem thus posed calls for investigation primarily on the plane of ideas and attitudes. Realistically speaking, there was nothing wrong with Justinian's expansionism. The weakness of the ruling families of Vandals and Goths offered inviting opportunities for intervention; Catholics could be thought to need rescue from heretical Arians; and there were even precedents for an attack on the Vandals in North Africa, against whom an expedition in 468 had expensively failed.⁶³ Imperial rule, however, also possessed dimensions that the language of political realism tended to undermine rather than to enhance. Other considerations than force had buttressed the long ascendancy of Rome in the world. This point is already made in recognizing that the term barbarian, with its connotations of inferiority, was a Greco-Roman invention that those whom it denoted could not help but apply to themselves. Besides, a ruler whom everyone for many centuries kept calling universal regardless of the condition of his armies enjoyed a daunting advantage. He left even hostile aliens—let alone friendly ones—at a loss to define their collective identity except in a relationship somehow dependent on his good will. The dilemma was compounded when such a monarch became the temporal head of a universal religion with millions of adherents guided by an educated clergy who daily led prayers for the perpetuation of his rule.⁶⁴ A legitimacy grounded in such intangible assets as these had subtler means of action at its disposal than generalship. More was needed to ruin it than an occasional defeat.

From the heights of their power and reputations, fourth-century emperors like Constantine and Theodosius had propagated ambitious and indiscriminate notions of their universality. Good emperors exemplified love of mankind, *philanthropia*, and were responsible for the welfare of barbarians as well as of their direct subjects.⁶⁵ The same Themistius who urged tax cuts also encouraged imperial concern for the lives of barbarians who, if spared, would prove as useful, in time, as the obedient taxpayers descended from foreigners conquered long ago. He declared to Theodosius I, "Vanquishing the barbarians by armed force resolves nothing if they are not saved afterwards by *philanthropia*, that is to say, unless a policy is afterwards devised that takes account of their existence and draws as much advantage from it as possible." Orosius's emphatically Christian version of universal history stressed how much the making of the empire had cost in barbarian as well as Roman blood.⁶⁶ Writing less than a decade

⁶³ For such practical considerations, see Bury, *History of the Later Roman Empire*, 2: 124–26, 151–69, 286. The attack of 468 was meant to support a candidate for the western emperorship, not to recover territory.

⁶⁴ A curious illustration of the problem of barbarian identity occurs in Justinian's summary of old Roman jurisprudence: an *obiter dictum* in the discussion of the rights of citizens while prisoners beyond the borders specifies that, before a *gens* can be regarded as an enemy, there must have been a relationship of *amicitia*, *hospitium*, or *foedus amicitiae causa factum* between it and the Roman state; Justinian *Digesta* 49. 15. 5. For fifth-century ecclesiastical conditions particularly favorable to the emperors, see Jean Gaudemet, *L'Église dans l'Empire romain* (Paris, 1958), 501–03.

⁶⁵ Glanville Downey, "Philanthropia in Religion and Statecraft in the Fourth Century after Christ," *Historia*, 4 (1955): 199–208. A sixth-century Byzantine treatise fails to specify whether or not imperial *philanthropia* still extended to barbarians; Patrick Henry III, "A Mirror for Justinian: The *Ekthesis* of Agapetus Diaconus," *Greek, Roman, and Byzantine Studies*, 8 (1967): 300–02, 306.

⁶⁶ Themistius *Oratio* 15 (A.D. 381), paraphrased by Dagron, "Le Témoignage de Thémistios," 104; and Orosius *Historia adversum paganos* 4–5 *passim*. Orosius's sympathy for the vanquished of long ago is a striking feature of his work, anticipating modern visions of Roman imperialism.

after the Goths of Alaric had sacked Rome, Orosius hailed the present era in which usurpers fell by God's intervention and even barbarians, when Christian, behaved with restraint; divine mercy was worthy of praise if the weakening of the empire was the condition for filling the churches of the east and west with Huns, Sueves, Vandals, and Burgundians who otherwise could not possibly be converted.⁶⁷ It is hard to believe that the same solicitude for barbarian lives, and for souls that could be saved by conversion, was altogether absent from the treaties of 382 and later that had given alien tribes a place in the empire as the alternative to wiping them out. Between 430 and 450, no less than three Greek historians looked back upon the events of the recent past and narrated them with confident approval: almighty God had smiled upon the descendants of Theodosius. This judgment cannot be ascribed to a narrowly Constantinopolitan perspective, for western events were not ignored. Throughout the world, according to these informants, the activities of all men, including barbarians, were continuously guided by the providence of a God who reserved His special favor for the pious youths occupying the imperial throne: "it is sufficient for an emperor to retain power merely by giving careful honor to the divine."⁶⁸

What could have happened after 450 to shake a serenity so firmly anchored as this in loftier considerations than physical force? It is true that the mid-fifth century had been the scene of hard military blows. The Vandal Gaiseric unexpectedly seized Carthage in 439 and continued until his death in 477 to direct the activities of hostile forces in the heart of the Mediterranean. From 441 to 454, Attila led the Huns in devastating attacks across the land frontiers of both east and west. Even though the empire survived these problems much as it had earlier ones, without lasting ill effects or major cessions of territory, it is tempting to think that, here if not before, there were genuine manifestations of barbarian strength, hostility, and determination.⁶⁹

Yet Gaiseric and Attila did not set a model of assertiveness to the western barbarians in the generations immediately following their deaths, and neither did the Constantinople of Justinian (527–65) trace its disillusionment with Roman universality to their exploits. Its eyes were on an earlier epoch. Two decades before Justinian took office, the historian Zosimus announced that, just as Polybius had narrated how Rome had risen to world domination in fifty-three years, so he would tell how it had descended from this pinnacle in an equal period of time. Zosimus never specified what span he had in mind, but Jordanes, a contemporary and admirer of Justinian, precisely identified it: for "almost sixty years," he said, until the advent of Marcian in 450, a succession of "dainty" em-

⁶⁷ Orosius *Historia* 5. 22 (current civil wars are milder), 7. 35. 6–9, 12–22 (God smites usurpers), 3. 23. 66–67, 7. 39 (restraint of Christian barbarians). Orosius also maintained that the *circumpositae gentes* had a positive role in God's plan—namely, to serve at His bidding as scourges of the empire when it sinned; *Historia* 7. 22. 6.

⁶⁸ The three Greek historians are Socrates, Sozomen, and Theodoret—that is, the orthodox continuators of Eusebius as distinct from the Eunomian Philostorgius. On their views, see Glanville Downey, "The Perspective of the Early Church Historians," *Greek, Roman, and Byzantine Studies*, 6 (1965): 57–70. On their western interests, see Walter E. Kaegi, *Byzantium and the Decline of Rome* (Princeton, 1968), 176–204. For the passage from Sozomen quoted here, whose subject is the western emperor Honorius, see Kaegi, *Byzantium and the Decline of Rome*, 186.

⁶⁹ For these events, see Bury, *History of the Later Roman Empire*, 1: 251–60, 271–76, 288–98, 323–41, 390, 410.

perors had diminished the realm.⁷⁰ These were the same emperors reigning from 395 onwards whom the church historians had hailed for “giving careful honor to the divine” and being correspondingly blessed by God. Procopius, another contemporary of Justinian, spelled out their responsibility for the loss of the empire: “Now, while Honorius was holding the imperial power in the West, barbarians took possession of his land; and I shall tell who they were and in what manner they did so”; a little later, Valentinian III “failed to recover for the empire anything of what had been wrested from it before, and he both lost Libya in addition to the territory previously lost and was himself destroyed.”⁷¹ Such a vision of the past, which Zosimus shows to have already been current under Justinian’s predecessors, gave ample justification for a policy of military “recovery” of the west. It also underscores the puzzling question how the faith of Constantinople in a continuing universality sustained by divine favor had turned in half a century into a grim belief in the judgment of battle.

A major aspect of the answer eludes our grasp. The church historians who wrote under Theodosius II were succeeded after 450 by a series of authors reviving the tradition of lay historiography; Priscus, the most distinguished among them, died after 472. Only fragments of their works survive in the collections of Photius and other Byzantine anthologists.⁷² The integral versions have perished, taking with them any possibility for us to discern a gradual change in mood and outlook that might have been occasioned by political and military circumstances.

Failing the comprehensive resources that these lay histories would have provided, ecclesiastical affairs offer the most promising evidence for detecting the movement of opinion. The same year 451 in which Attila retreated from his western adventure also witnessed the great episcopal gathering at Chalcedon, where the bishopric of Rome joined hands with that of Constantinople to impose orthodoxy upon the church concerning the two natures of Christ. Marcian, the emperor who gathered this council and determined its outcome, earned a place of the utmost honor in the historical records of Constantinople for both “treading on the necks of all of his enemies” and instituting true Christian doctrine.⁷³ His foreign foes, however, proved to be more lastingly buried than religious controversy. As Peter Brown has recently pointed out, the closing decades

⁷⁰ Zosimus *Historia nova* 1.1, 1.57.1. Jordanes *Romana* 332: “Regnum quod delicati decessores predecessoresque eius per annos fere sexaginta vicissim imperantes minuerant . . . reparavit.” Fifty-five years from Honorius and Arcadius (395) to the death of Theodosius II (450), sixty to that of Valentinian III (455), the last male of the Theodosian dynasty. To my knowledge, no one has yet noticed the agreement between Jordanes’s figure and that of Zosimus (the unfinished *Historia nova* trails off in 410). See the bemused speculations of François Paschoud’s *Cinq études sur Zosime* (Paris, 1975), 187–206.

⁷¹ Procopius *Bellum Vandalicum* 1.2.1, 1.3.12, trans. H. B. Dewing (Loeb), *Procopius*, 2 (London, 1916): 9, 27.

⁷² The fragments have been assembled and translated by C. D. Gordon; see his *The Age of Attila: Fifth-Century Byzantium and the Barbarians* (Ann Arbor, 1960). For the works of these authors and their equally vanished ecclesiastical counterparts, notably the chronicle of Eustathius of Epiphania, see Wilhelm von Christ *et al.*, *Geschichte der griechischen Literatur*, pt. 2, 2d half (6th ed., Munich, 1924): 1034–39, 1443–44, 1483–85.

⁷³ On Chalcedon, see, for example, Stein, *Histoire du Bas-Empire*, 1: 309–15. On Marcian, see Jordanes *Romana* 332; and, for a eulogy that stresses his ecclesiastical role, see Evagrius *Historia ecclesiastica* 2.1. Interestingly enough, Marcian also earned Zosimus’s praise by implication (execration of the *folles senatorius*, which Marcian abolished); see *Historia nova* 2.38.4.

of the fifth century are less noteworthy for the unraveling of the Chalcedonian settlement than they are for the unshakable determination of the bishops of Rome to resist every imperial attempt to attain religious peace by compromise; while "the emperors [of Constantinople] hoped to make the partisan bishops and their flocks live up to standards of unity and obedience that were patently being realized in every other field but religion," the leadership of the Catholic church of the west profited from the physical security it enjoyed in lands precariously governed by Arian generals to oppose all modifications of Chalcedonian teachings, regardless of the cost to Christian universality.⁷⁴ Emperors who were forced to endure such defiance could scarcely sustain the illusion that the barbarian settlements in the west had left their ecumenical dominion unimpaired. The will that thwarted them had nothing alien about it; enough of their nearby subjects championed Chalcedon, and drew strength from the support of old Rome, to assure the emperors of the domestic nature of their problems. But nationality hardly made a difference. Now that God's favor was in serious doubt, official Constantinople could take no more comfort in the reverence still inspired by the imperial name among Latin-speaking Catholics and their governors; the assets for *Romanitas* that existed in the west at the opening of the 500s were irrelevant to the court. On the contrary, Byzantium had greater confidence to gain from believing that the west had long been "lost" to barbarians than from precisely defining the nature of the resistance that stood in the way of imperial designs.

In the next generation, the undermanned expeditions launched by Justinian achieved a few durable annexations to the empire. They also brought greater ruin to Italy than did all of the invaders of the previous one hundred and fifty years. Yet their most startling novelty was in the sphere of religion. The popes of conquered Rome were taken in hand and kept in Constantinopolitan custody until the emperor's wishes for orthodoxy were irrevocably espoused. Marcian's exploit of 451 was supposed to repeat itself in 553 at the second council of Constantinople; again, though by very different means, Rome joined with the east in the declaration of doctrine: by the condemnation of the Three Chapters, the decision of Chalcedon was modified in a way that, Justinian hoped, would produce religious peace. The effect of the council in the east was undramatic. The west, however, acquired something that it had never experienced before—a schism that separated large segments of the Latin episcopate from its spiritual leader.⁷⁵ In time, the papacy not only outlived the repudiation it suffered for joining with Justinian in condemning the Three Chapters but even

⁷⁴ Brown, *The World of Late Antiquity* (London, 1971), 146, 145–48. For the terms of Roman resistance, see Gaudemet, *L'Église dans l'Empire romain*, 503–04; and, for the events, see Stein, *Histoire du Bas-Empire*, 2: 20–28, 182–92.

⁷⁵ Louis Duchesne, *L'Église au sixième siècle* (Paris, 1925), 109–258; and Émile Amann, "Trois-chapitres," in *Dictionnaire de théologie catholique*, 15, pt. 2 (Paris, 1950): 1868–1924, esp. 1910–17. The best sense one can acquire of the importance of the Three Chapters controversy in the west is by taking stock of the literature it generated, including a determined effort to absorb into the Latin tradition the hitherto little-known fifth-century religious history of the east.

convinced an ill-educated Latin church that it had been right to do so.⁷⁶ There was, nevertheless, an interval of hesitation, and it lasted long enough for the stoutly Chalcedonian westerners to reconsider where their immediate loyalties lay. In a world in which the temporal head of the church actively promoted heresy, the mandate of heaven might well appear to have descended upon rough-hewn kings who venerated the saints, left religious orthodoxy to bishops, and were even known, upon occasion, to take the field against the enemies of the faith.

The histories composed by Gregory of Tours from 575 to 593 are the outstanding evidence for the reconciliation of Latin aristocrats to an alien regime. To a pious Gallo-Roman bishop like Gregory, living when he did, a brutal slayer of relatives like the Frank, Clovis, seemed nevertheless to merit such high esteem that his living successors were to be rebuked for departing from the laudable example he had set.⁷⁷ Much the same process is illustrated in Visigothic Spain by John of Biclar. Though of Gothic descent, John was a Catholic and educated in Constantinople; he returned to Spain after at least seven years, in the same decade in which Gregory of Tours began to write. John chose to continue a chronicle that is heavily committed to defending the Three Chapters from Justinian's, and the pope's, condemnation. This helps explain why the Arianism of the Visigothic king Leuvigild (567–80) was no obstacle to John's emphatic praise of his reign and celebration of its effectiveness: "He wondrously restored the territory of the Goths . . . to its ancient boundaries."⁷⁸ A third historian, Marius of Avenches, allows us to see how such a reorientation of sympathies affected portrayals of the past. According to Marius, the Burgundians' move into Gaul in the 450s involved their harmoniously "partitioning the lands with the Gallic senators."⁷⁹ By their active support of barbarian kings, these late sixth-century historians lend weight to the opinion of K. F. Stroheker and other modern scholars who have urged that the turn from antiquity to Middle Ages be set in the vicinity of A.D. 600.⁸⁰

Although such an argument has merit, there is no denying that the turning point that has long had the widest currency falls considerably earlier than the

⁷⁶ A good indicator of the papacy's success in blotting out its role is found in Paul the Deacon's brief and remarkably distorted account of the controversy, written the late eighth century; see his *Historia Langobardorum* 3. 20, 26.

⁷⁷ The paradox of Gregory's admiration for Clovis is epitomized by the famous eulogy that he provocatively situated in a context of brutality; *Historiae* 2. 40–42. For his rebuke of later Merovingians, see the Preface to *Historiae* 5.

⁷⁸ John of Biclar *Chronicon* under 569, ed. Theodor Mommsen, *MGH, Auctores antiquissimi*, 11: 212. On John and his context, see J. N. Hillgarth, "Coins and Chronicles: Propaganda in Sixth-Century Spain and the Byzantine Background," *Historia*, 15 (1966): 483–508; and Jacques Fontaine, "Conversion et culture chez les Visigoths d'Espagne," *Centro italiano di studi sull'alto medioevo, Settimane di studio*, 14 (1967): 108–15. Neither of these excellent studies takes into account the divisive effects on western Catholics of the Three Chapters controversy.

⁷⁹ Marius of Avenches *Chronicon* under 456, ed. Theodor Mommsen, *MGH, Auctores antiquissimi*, 11: 232. Marius was also a contemporary of Gregory of Tours. For a discussion of this passage, see my *Barbarians and Romans (A.D. 418–584)*, chap. 4 n. 9. Religious considerations aside, it also seems noteworthy that Gregory, John, and Marius started to write very soon after the Lombards had wrested much of Italy from Byzantium.

⁸⁰ Stroheker, "Um die Grenze zwischen Antike und abendländischem Mittelalter," *Saeculum*, (1950): 440–42, 464–65, with reference to Alfred von Gutschmid (1863).

age of Gregory of Tours and Gregory the Great. The place in this transition assigned to Alaric and Attila, Aetius and Theodoric, and other worthies of the fifth century is too great to justify so late a beginning for the Dark Ages as A.D. 600. Our history books make much of aliens taking Rome in 410 and 455, of the creation of "barbarian kingdoms," and of the hostile cunning of Gaiseric the Vandal. We even have narratives, enriched by maps of our devising, that chart the slow descent of barbarians from northern lands to positions of prominence in what had been Rome's world.⁸¹ These elements do not constitute the past as conceived of by a westerner like Gregory of Tours, let alone the Briton Gildas, but neither are they merely figments of later medieval and modern European imaginations.⁸² The last question for us to consider is where this panorama of a mounting barbarization of the west has come from.

Once the problem is phrased in these terms, it ceases to be surprising that the same metropolis of Constantinople in which the ruin of the Roman Empire began to be affirmed on the eve of Justinian's accession was also the place that, in his reign, gave birth to the first circumstantial evocations of the "barbarian west." Negligent emperors of a dynasty long dead were held responsible, as we have seen, for letting lands slip from their grasp, but antagonists worthy of respect were needed to profit from their foolishness and loose living. A historian might hope, if ambitious, to detect general causes for the decline of world dominion; this was the course that Zosimus attempted to take by placing Christian emperorship itself on trial, together with its philobarbarism.⁸³ The more familiar and popular approach was to dramatize the events in the personal terms of biography and anecdote. The result reaches us in the chronicle of John Malalas, in the introductory chapters of Procopius, and, most fully, in the writings of Jordanes, the Byzantine Catholic of Gothic blood who composed in Latin the original history of a whole barbarian people. The place of heroic foreigners in the narratives of Procopius and Jordanes is too widely known to require comment; here are the fifth-century worthies in already epic dimensions.⁸⁴ The muddled chronology that was long to characterize the history of the 400s, fusing or interchanging one hero with another according to the needs of each tale, is also well

⁸¹ Concerning maps, see note 28, page 284, above. It would be interesting to establish whether their prototype antedates the nineteenth century.

⁸² Gildas took Britain as his background, with its rule by Rome merely an interval, though an admired one, in its historical existence; Gildas *De excidio* 3–20. Gregory started with the Creation but quickly carried his narrative to the Christianization of Roman Gaul in the third century, after which Christian Gaul remained his focus; for the start of the narrowed focus, see *Historiae* 1. 29.

⁸³ See my "Zosimus, the First Historian of Rome's Fall," 414–16. This feature made Zosimus, since Löwenclav (1576), particularly appealing to modern tastes, as Mazzarino has pointed out; *End of the Ancient World*, 92–99.

⁸⁴ John Malalas *Chronographia*, ed. L. Dindorf (Bonn, 1831); Malalas deserves to be more widely known. Concerning the introductory chapters of Procopius, see my *Barbarians and Romans (A.D. 418–584)*, chap. 3. Also see Jordanes *Getica*, trans. C. C. Mierow, *The Gothic History of Jordanes* (Princeton, 1915); Mierow's introduction and notes are somewhat dated. It has been widely believed that the *Getica* mirrors, when it does not deprave, the lost Gothic History of Cassiodorus; for a forceful statement of this argument, see Arnaldo Momigliano, "Cassiodorus and the Italian Culture of His Time" (1955), reprinted in his *Studies in Historiography* (New York, 1966), 191–99. Without, to be sure, disputing Jordanes's use of Cassiodorus's History, I believe in the originality of Jordanes's version and consider it comparable to the originality of Zosimus.

established in these accounts. Attila, for example, though barely a century dead, was condemned to waging two unsuccessful campaigns against Gaul instead of one.⁸⁵ Earlier epochs were even more loosely handled: "While [the emperor Gallienus (d. 286)] was given over to luxurious living of every sort . . . , the Goths recrossed the strait of the Hellespont, . . . sacking Troy and Ilium on the way. These cities, which had scarce recovered a little from the famous war with Agamemnon [that is, the Trojan War] were thus destroyed anew by the hostile sword."⁸⁶ The message of Jordanes, like that of Procopius, would have been only blunted by accurate measurements of time.

Barbarian leaders celebrated by Constantinopolitans for their exploits against degenerate emperors also deserved a coherent ethnic context and an ancient history. When scholars of the Carolingian renaissance became aware of a wider Germanic identity embracing Scandinavians as well as Franks, Saxons, and Bavarians, they were quick to grasp how great a contribution the history of Jordanes made to their knowledge of a common past.⁸⁷ And, when modern Germans sought to trace the wanderings of their tribal forebears, they prized Procopius for the links he disclosed between the men who had established kingdoms on the shores of the Mediterranean and their homelands far to the north.⁸⁸

There were many Gothic nations in earlier times, just as also at present, but the greatest and most important of all are the Goths, Vandals, Visigoths, and Gepaedes. . . . All these, while they are distinguished from one another by their names, as has been said, do not differ in anything else at all. For they all have white bodies and fair hair, and are all tall and handsome to look upon, and they use the same laws. . . . As it seems to me, they all came originally from one tribe, and were distinguished later by the names of those who led each group.⁸⁹

So began his sketch of the "migration of peoples," remarkable in every respect as an anticipation of Ludwig Schmidt's *Geschichte der deutschen Stämme* (2d ed., 1941) and its progenitors. Jordanes added the details that allowed this tale to be traced as far back as it would go until the invention of Indo-Germanic philology—namely, to the "island of Scandza, . . . workshop of races [and] womb

⁸⁵ Jordanes *Getica* 181–218, 225–27, which is paralleled by Procopius *Bellum Vandalicum* 3. 4. 24, 29. Malalas *Chronographia*, ed. Dindorf, 358–59: Aetius rallied Alaric to fight Attila the Gepid. The development of these confusions is easily detected in Byzantine and later Latin chronicles, where, for example, Alaric's sack of Rome gets conflated with Gaiseric's, Aetius and Theodoric the Ostrogoth switch identities, and so forth. In the twelfth century, Otto of Freising protested against accounts that made Theodoric contemporary to Ermanaric and Attila; he might have had in mind the eleventh-century Annals of Quedlinburg. The curiosity is how fully these tendencies are already apparent in Procopius's work; for example, Arcadius is airily described as living "some time earlier" than Odoacer.

⁸⁶ Jordanes *Getica* 107–08, trans. Mierow, 81–82.

⁸⁷ Zöllner, *Die politische Stellung der Völker im Frankenreich*, 46–47, 52. The main exhibit is Freculf of Lisieux's chronicle, in *Patrologia Latina* 106: 967 (Frankish origins). The further influence of Jordanes on Freculf is apparent in his last books; see *Patrologia Latina* 106: 1203–58.

⁸⁸ An extraordinarily important part in modern conceptions of Germanic migrations is played by two passages of Procopius, both of dubious historicity: *Bellum Vandalicum* 1. 22. 1–12, and *Bellum Gothicum* 2. 15. See Ludwig Schmidt, *Geschichte der deutschen Stämme bis zum Ausgang der Völkerwanderung: Die Ostgermanen* (2d ed., Munich, 1941), 107, 535–55; and Ernst Schwarz, *Germanische Stammeskunde zwischen den Wissenschaften* (Constance, 1967), 51–52.

⁸⁹ Procopius *Bellum Vandalicum* 1. 2. 2–5, trans. Dewing, 9–11.

of nations.”⁹⁰ The resources of Greco-Roman learning at the command of Justinian’s contemporaries were entirely adequate to evoke a majestic panorama that no one before had ever seen. What its authors could hardly have imagined was the amazing fortune that their vision of the barbarian past would enjoy among future generations. Perhaps the most enduring conquest of the age of Justinian was the one that its historians were allowed to make by a posterity whose longing for ancient roots far from the Mediterranean disposed it to ignore the curiously Byzantine source of such wisdom.⁹¹

WHEN ALFONS DOPSCH SAID, “The conquest of the Roman Empire took place on different lines from the conquest of other states in political history,” he expressed in weak language a dilemma that many other historians have confronted in late antiquity.⁹² How can one fail to be puzzled? A great empire was bordered by fragmented neighbors whose turbulence it had kept for centuries from getting out of hand; its western provinces came under alien control with frequent assistance from the imperial government; the steps involved in bringing about the fall of the empire were so gradual that the process extended over two hundred years; and, even after imperial rule had gone, the remaining Romance population and its culture had a dominant part in the early medieval development of the west. To more than one author, “conquest” hardly seemed the right word to express so prolonged and ambiguous a process. The chronicle of St. Jerome notes a Roman success of 373 against barbarians on the Rhine frontier: “The Saxons were slaughtered at Deutz in the region of Franks.” Another Christian Latin author, three and a half centuries later, portrayed a slightly different cast of characters acting out the very same drama: “When the fervently pagan Saxons . . . rebelled, Charles [Martel] called up the Frankish army, came to the place where the Lippe joins the Rhine [about fifty miles north of Deutz], quickly crossed it and laid waste to that region with frightful thoroughness.”⁹³ In view of such persistence in behavior, is there much point in our believing that the imperialism of Rome had withdrawn over the centuries to a solitary perch in Constantinople?

If historical change is what we are looking for, another dimension of events needs to enter our range of vision. Many thoughtful men—ancient, medieval, and modern—have shared our conviction, however implausible it may be, that “the crimes, follies, and misfortunes of mankind” do indeed form intelligible and meaningful patterns. Our opportunity to come to terms with change lies

⁹⁰ Jordanes *Getica* 25, trans. Mierow, 57. I have made a small change in the translation.

⁹¹ For this longing, see note 31, page 285, above. For an important account of the historiography of the *Völkerverwanderung*, see Hans Messmer, *Hispania-Idee und Gotenmythos* (Zurich, 1960), 9–12, 43–57. The modern version was launched by the Viennese physician and Habsburg court historian Wolfgang Lazius; see his *De gentium aliquot migrationibus* (Basle, 1557). Each of the twelve books of Lazius’s work is assigned to one tribal group, with an appropriate illustration of ethnic costume.

⁹² Dopsch, *Economic and Social Foundations of European Civilization*, 386.

⁹³ Jerome *Chronicon*, ed. Rudolf Helm (Berlin, 1956), 246; and Fredegar *Chronica, Continuationes* 19, ed. and trans. J. M. Wallace-Hadrill (London, 1960), 93. I have made minor changes in the translation of Fredegar. The same phenomenon can be set in archaeological perspective; see Edward James, “Cemeteries and the Frankish Settlement in Gaul,” in P. H. Sawyer, ed., *Names, Words, and Graves* (Leeds, 1979), 55–89.

principally in the wondrous variety and mutability of human opinion. In the context of this essay, a pertinent example concerns Alaric's seizure of Rome in 410. The event immediately moved Jerome and Augustine to speak, and the magnificent flow of their oratory would assure us that the incident had few equals in world history even if neither one had expressly said so. Besides, a careful near-contemporary record of the circumstances was set down by Olympiodorus, and, though this is lost, major extracts survive in Sozomen and elsewhere. A century or so after Sozomen, however, when the church historians were turned into Latin, the account of 410 chosen for translation was not his but the brief, oversimplified summary of Socrates.⁹⁴ Even this was truer to fact than the almost comic version already current in Justinian's empire, which begins as follows: the people of Rome rioted, forcing the emperor Honorius to withdraw to Ravenna; enraged, he called General Alaric to come from Gaul with his troops to plunder the capital; Alaric came as bidden, but, instead of obeying Honorius, he conspired with the Romans, stole the emperor's treasure and sister, returned to Gaul, and so forth. This cheerful legend was the form *par excellence* in which Byzantium chose to remember 410.⁹⁵ Naturally, there is much more to the event than this. It suffices to remember that Alaric's deed, misdated to 412, was central to Flavio Biondo's theme of the *inclinatio imperii Romani*, itself stated near the start of a line of reflection and study that culminated in Gibbon's *History of the Decline and Fall of the Roman Empire*.⁹⁶

The present essay has sought to interpret the role of the barbarians in late antiquity by integrating the record of human opinions more closely with the unfolding of events than was done, for example, by Pierre Courcelle's important *Histoire littéraire des grandes invasions germaniques*.⁹⁷ Guided by a pattern of "ruin" suggested by Sulpicius Severus at the very time when danger threatened, it has explored the idea that "the entrance of the Germans into history" can be charted by the place they held, as barbarians or whatever, in the surviving utterances of contemporary observers. For the years from Constantine to the Lombard invasion of Italy—the age when Rome fell—our attention has centered on four questions: Why did the emperors respond more peacefully to barbarian attacks after 376 and 406 than their predecessors had in the third century? Why was it the western provinces of the empire that received autonomous barbarian garrisons? How did Constantinople arrive at the belief that the west had been lost to barbarian rulers? And where was our image of the earliest "barbarian west" originally drawn? The bond between such questions is that each one forces us to look elsewhere for answers than among the nonliterate barbarians.

The solutions found to these problems are neither complete nor final. To

⁹⁴ J. F. Matthews, "Olympiodorus of Thebes and the History of the West (A.D. 407–425)," *Journal of Roman Studies*, 60 (1970): 79–97. Cassiodorus-Epiphanius *Historia ecclesiastica tripartita* 11. 9 (written ca. 570).

⁹⁵ The legend is first found in Malalas *Chronographia*, ed. Dindorf, 349–50. It was known to Procopius, who believed it to be out of keeping with Honorius's character; *Bellum Vandalicum* 1. 2. 10. It is repeated, for example, by John of Nikiu, Theophanes, Zonaras, and Nicephorus Calistus.

⁹⁶ Mazzarino, *The End of the Ancient World*, 77–82; and Christopher Dawson, "Edward Gibbon," *British Academy, Proceedings*, 20 (1934): 159–80.

⁹⁷ Courcelle, *Histoire littéraire des grandes invasions germaniques* (1948; 3d ed., Paris, 1964). Courcelle seems never to have considered where his notion of the early Germans came from.

summarize them by way of conclusion would tend to suggest that simple formulas may be substituted for the complexities of three centuries. It is more appropriate to close by spelling out the premises that have governed the method rather than the substance of this inquiry. They are, in brief, that an empire whose influence was so profound and enduring as Rome's has to be approached with extraordinary respect for its solidity;⁹⁸ that we need to be guarded and very neutral in speaking about the "barbarian" neighbors of the empire as long as they are beyond our powers to grasp on terms of their choice; and, finally, that the best defense against anachronism is for us not only to be sensitive to the origin of our sources of information but also to integrate their perceptions into our vision of the events they commemorate. Even though the discussion ought never to end, it may be possible, at least for a while, to agree on the point of departure.

⁹⁸ Sir Ronald Syme, *Colonial Elites: Rome, Spain, and the Americas* (London, 1958), 4: "What Rome achieved tends to be ignored."

Habsburg Absolutism and the Cortes of Castile

CHARLES JAGO

THE DECADE of the 1640s was a period of profound instability in Western Europe as a succession of states succumbed to rebellion and civil war. Given the ubiquity and relative simultaneity of these political seisms, historians have sought to discover common patterns of causation. In one masterful survey of the subject, T. K. Rabb has traced the origins of the "crisis of the seventeenth century" back to the early sixteenth century, linking it to a multiplicity of revolutionary changes—cultural, social, religious, political, and economic—that occurred at that time.¹ But the political upheavals of the mid-seventeenth century need not be assigned such a distant paternity; a more immediate cause was the Thirty Years' War (1618–48).

This prolonged and devastating conflict in the very heart of the European Continent created a vortex into which all states were inexorably drawn. Military and diplomatic costs escalated at a time when the onset of recessionary economic conditions attenuated the financial resources that governments could command. As a consequence, monarchs were forced to expand their fiscal powers and press to the full their absolutist claims. The pressures conducive to more active and interventionist government were offset by the conservative forces unleashed by those with vested interests to protect. Governments advocating change clashed with a complex of different and not always compatible social groups committed to preserving the status quo. In certain instances, the leadership of this opposition fell to institutions that not only enjoyed a degree of independence from their ruling monarchs but could also legitimately claim to represent the interests of the body politic. The English Parliament, the French *parlements*, the Estates General of the United Provinces, and the *Diputacio* in Catalonia came to the fore in this confrontation between rulers and the ruled. Their active participation proved their ability to transform an often diffuse discontent into sharply focused and highly volatile constitutional conflicts.²

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¹ Rabb, *The Struggle for Stability in Early Modern Europe* (New York, 1975), 35–48.

² There is an extensive literature on the "crisis of the seventeenth century." For some of the major works,

One country that stood as a notable exception to this general political crisis was Castile—the mainstay of Imperial Spain. If anything, Castile had greater cause than other European societies to challenge the crushing authority of the state. It was mercilessly taxed and its manpower was continuously drained to supply the needs of Spanish armies abroad. In addition, it suffered from recurring famine and plague and experienced a pronounced degree of depopulation. A sense of malaise, disillusionment, and cynicism pervaded Castile—so much so that one historian has compared the “age of Don Quijote” to the “age of Charlie Chaplin.”³ Despite these tribulations, Castile remained in a state of political calm, although some might consider “stupor” to be a more appropriate term. Writing of the 1640s, J. H. Elliott has observed that “one of the most remarkable features of this decade was that the heart of the [Spanish] monarchy, Castile, held firm. For all the burdens and misfortunes of war, there was no Fronde in Madrid.”⁴ The monarchy did not, of course, go unchallenged; the opposition that did emerge, however, never approximated, either in scope or in intensity, the forces undermining the stability of other European states.⁵

HOW DID CASTILE ESCAPE SO LIGHTLY this period of widespread political disorder? The historiography of Habsburg Spain provides a simple but misleading answer. By the seventeenth century, it is generally argued, royal absolutism had triumphed in Castile. Compared to the eastern principalities of Spain—Aragon, Catalonia, and Valencia—where the authority of the monarchy was tightly circumscribed by provincial liberties and by the powers held by a relatively cohesive and self-aware elite, Castile offered few limitations and little organized opposition to Habsburg rule.⁶ The immensely wealthy and potentially disruptive Castilian aristocracy had been neutralized by the clever use of royal patronage and favoritism and by the toleration accorded to its seigniorial properties and

see Trevor Aston, ed., *Crisis in Europe, 1560–1660* (New York, 1966); R. Forster and J. P. Greene, eds., *Preconditions of Revolution in Early Modern Europe* (Baltimore, 1970); Henry Kamen, *The Iron Century: Social Change in Europe, 1550–1660* (New York, 1971); Geoffrey Parker and Lesley M. Smith, *The General Crisis of the Seventeenth Century* (London, 1978); R. B. Merriman, *Six Contemporaneous Revolutions* (Oxford, 1938); and Rabb, *The Struggle for Stability in Early Modern Europe*.

³ Pierre Vilar, “El tiempo del ‘Quijote,’” in *Crecimiento y desarrollo* (Barcelona, 1964), 431–48. For the best essays in English on the “decline of Spain,” see J. H. Elliott, “The Decline of Spain,” *Past & Present*, no. 20 (1961): 52–75, and “Self-Perception and Decline in Early Seventeenth-Century Spain,” *ibid.*, no. 74 (1978): 41–61; Earl J. Hamilton, “The Decline of Spain,” *Economic History Review*, 1st ser., 8 (1938): 168–79; Henry Kamen, “The Decline of Castile: The Last Crisis,” *ibid.*, 2d ser., 17 (1964–65): 63–76, and “The Decline of Spain: A Historical Myth,” *Past & Present*, no. 81 (1978): 24–50; and R. A. Stradling, “Seventeenth-Century Spain: Decline or Survival,” *European Studies Review*, 9 (1979): 157–94.

⁴ Elliott, “Revolts in the Spanish Monarchy,” in Forster and Greene, *Preconditions of Revolution in Early Modern Europe*, 109.

⁵ Antonio Domínguez Ortiz, *Alteraciones Andaluzas* (Madrid, 1973), and “La Conspiración del Duque de Medina Sidonia y el Marqués de Ayamonte,” *Archivo Hispalense*, no. 106 (1961): 133–59; and José Antonio Maravall, *La Cultura del Barroco: Análisis de una estructura histórica* (Barcelona, 1975), 98–127, and *La Oposición bajo las Austrias* (Madrid, 1973).

⁶ Two outstanding studies on the relations between the Spanish crown and the eastern Iberian principalities during the seventeenth century are James Casey, *The Kingdom of Valencia in the Seventeenth Century* (Cambridge, 1979); and J. H. Elliott, *The Revolt of the Catalans: A Study in the Decline of Spain (1598–1640)* (Cambridge, 1963).

prerogatives. Of equal importance, the Castilian Cortes, or parliament, from which the aristocracy was effectively excluded, had been deliberately and systematically reduced to total submissiveness.⁷ As a result, it lacked the inclination and ability to grasp the oppositional role seized by other parliamentary institutions elsewhere in Western Europe.

Where it relates to the Cortes of Castile, this analysis introduces a historical orthodoxy deeply ingrained in Spanish constitutional history. Guided by the premises first articulated by Spanish liberals during the era of the French Revolution, historians have accepted the argument that the Castilian Cortes attained the peak of its prestige and influence during the thirteenth and fourteenth centuries and that thereafter it steadily declined.⁸ The early liberals, such as Martínez Marina, believed that the medieval Cortes effectively embodied the principle of popular sovereignty. While few historians still subscribe to this viewpoint, most continue to maintain that the medieval Cortes reflected the vigor, confidence, and incipient democratic tendencies inherent within the emerging urban life of Reconquest Castile. Its strength and independence, so the argument runs, began to be gradually eroded from the fourteenth century on with the spread of royal control over municipal governments, the concurrent rise of closed and venal urban oligarchies, and the restriction of representation in the Cortes to two delegates (*procuradores*) selected, usually undemocratically, from a privileged group of eighteen cities. The *coup de grâce*, according to this narrative, came with the defeat of the rebellious *comuneros* in 1521, at the outset of the reign of Emperor Charles V. The consequent eradication of a militant, urban-based opposition to Habsburg rule and the consolidation of a firm alliance between the crown and the aristocracy removed all barriers to the advance of monarchical authoritarianism.

This traditional thesis is blessed with the twin virtues of simplicity and linearity; rise and decline sequences are always attractive. Nevertheless, it has not been immune to criticism. The history of the medieval Cortes has undergone substantial revision during the past three decades to the point that the alleged vigor and independence of the institution have been called into question.⁹ Simi-

⁷ Charles V expelled the "aristocratic estate" from the Cortes of 1538 when it refused to concede a new indirect tax. J. H. Elliott has attached exceptional importance to this event: "The tax exemption of the nobles . . . had been preserved at the cost of destroying the last hopes of constitutionalism in Castile. After 1538 . . . the representatives of the towns were compelled to fight single-handed a losing battle against the increasingly arbitrary demands of the crown." Elliott, *Imperial Spain, 1469-1716* (London, 1963), 196. On the relations between the crown and the aristocracy during this period, see Charles Jago, "The Influence of Debt on the Relations between Crown and Aristocracy in Seventeenth-Century Castile," *Economic History Review*, 2d ser., 26 (1973): 218-36, and "The 'Crisis of the Aristocracy' in Seventeenth-Century Castile," *Past & Present*, no. 84 (1979): 60-90.

⁸ The "classic" studies of the history of the Castilian Cortes are Francisco Martínez Marina, *Teoría de las Cortes*, 3 vols. (Madrid, 1813); M. Sempere, *Histoire des Cortes d'Espagne* (Bordeaux, 1815); D. Manuel Colmeiro, *Cortes de los Antiguos Reinos de León y de Castilla*, 2 vols. (Madrid, 1883); and W. Piskorski, *Las Cortes de Castilla en el período de tránsito de la Edad Media a la Moderna, 1188-1520*, trans. C. Sánchez Albornoz (Barcelona, 1930). For the most recent overview of the history of the Castilian Cortes, see José Manuel Pérez-Prendes, *Cortes de Castilla* (Barcelona, 1976). The flavor of Spanish liberal historiography is best captured in English by R. B. Merriman, "The Cortes of the Spanish Kingdoms in the Later Middle Ages," *AHR*, 16 (1910-11): 476-95.

⁹ Julio Valdeón Baroque, "Las Cortes medievales castellano-leonesas en la historiografía reciente," in Piskorski, *Las Cortes de Castilla* (2d ed., Barcelona, 1977), v-xxxii.

larly, the standard treatment of Charles V, whom liberal historians condemned vitriolically for subverting Castilian liberties and parliamentary traditions, has failed to withstand close scrutiny.¹⁰ But here revisionism ends. Beyond the Revolt of the *Comuneros* and its immediate aftermath, the history of the Cortes has been generally ignored, and, by default, the traditional liberal rendition of Castilian parliamentary history continues to prevail. The Cortes is still regarded as a peripheral institution of slight political significance.¹¹ The *procuradores* are usually depicted as voracious opportunists addicted to the bribes and favors given to reward their tractability. Some historians have contemptuously dismissed them for having conceded a seemingly endless succession of subsidies to the crown without removing in return blatant abuses of governmental authority. A few have recently started to question this accepted opinion, but their skepticism has yet to mature into an alternative thesis.¹²

Yet skepticism is warranted. The standard version of the history of the sixteenth- and seventeenth-century Castilian Cortes is based on a paucity of fact and on painfully inadequate research and is, therefore, both superficial and erroneous. The Cortes was not politically inconsequential during this period. It had the right to approve extraordinary taxes, thus depriving the crown of fiscal autonomy—an ingredient normally considered to have been essential to royal absolutism. The principle of no taxation without consent gave the Cortes and the eighteen cities it represented the ability to block and frustrate the interests of the crown and placed them in a strong position to negotiate tax agreements favorable to their own. During Philip III's reign (1598–1621), the urban patricians exploited this advantage: they insisted on retaining administrative control of the new taxes they conceded; they attempted to impose budgets to ensure that public monies were spent in the public good, and they demanded other conditions in the form of legislation and restrictions on the use of the royal prerogative, as their price for approving new subsidies. In effect, they began to distinguish between public revenues and the king's private funds and to exercise, if only in a limited and tentative fashion, the power of the purse. Their pretensions were finally countered directly by Philip IV (1621–65) and his favorite and prime minister, the Count-Duke of Olivares (1621–43). Unwilling to accept the limitations on monarchical authority implied by the powers held by the Cortes and cities and by the demands of their most ardent spokesmen, Philip and Olivares contrived to weaken and circumvent those vestigial parliamentary traditions that, contrary to orthodox historical opinion, enjoyed a brief and significant revival in late sixteenth- and early seventeenth-century Castile.

¹⁰ A. Cánovas del Castillo, "Carlos V y las Cortes de Castilla," *La España moderna*, 1 (1889): 73–115; and F. de Laiglesia, *Estudios históricos (1515–1555)*, 1 (Madrid, 1918): 299–495. This revisionist position has recently been developed further; see Charles David Hendricks, "Charles V and the Cortes of Castile: Politics in Renaissance Spain" (Ph.D. dissertation, Cornell University, 1976). H. G. Koenigsberger has also devoted a chapter to the Castilian Cortes during this period; see his *Estates and Revolutions* (Ithaca, 1971), 181–90.

¹¹ See, for example, J. A. Maravell, *Estado moderno y mentalidad social, siglos XVI a XVII*, 1 (Madrid, 1972): 356–64.

¹² A. W. Lovett, *Philip II and Mateo Vázquez de Leca: The Government of Spain (1572–1592)* (Geneva, 1977, 103–04; Felipe Ruiz Martín, "La Banca de España hasta 1782," in *El Banco de España: Una Historia económica* (Madrid, 1970), 73–96; and I. A. A. Thompson, *War and Government in Habsburg Spain, 1560–1620* (London, 1976), 275–76.

This moment of parliamentary success and the political controversies it occasioned have been overlooked by historians, primarily because they have been blinded by inherited presuppositions. They have tended to judge the performance of the Castilian Cortes according to modern liberal-democratic principles.¹³ Because it lacked legislative power, because its membership was neither democratically elected nor broadly representative, because it refused to adopt—except on rare occasions—an openly adversarial position toward the crown, the Cortes has been considered inherently defective. But these anachronistic retrojections have little value. The Cortes was not, either by design or intention, anything like a modern type of parliamentary institution. Although it wished to influence royal legislation, it did not strive to secure direct legislative power. Likewise, it did not consider its responsibilities to include the detailed scrutiny of government policies, and only on rare occasions did it openly challenge the crown. As the *procuradores* interpreted their principal function, they were to cooperate with the monarchy by conceding subsidies, although they intended that specific grievances should be redressed in return. Nevertheless, supply almost always took precedence over redress, and from the 1560s to the 1660s the Cortes met regularly for the primary purpose of debating and approving extraordinary subsidies. Historians have mined the minutes of these meetings for the evidence they afford on the decline of Spain, but their significance to the political and constitutional history of Castile has been generally ignored.¹⁴ Yet the *Actas*, as they are called, clearly attest to the fact that an understanding of the interaction between the Cortes and the crown concerning questions of taxation offers a penetrating insight into the political and constitutional development of early modern Castile.

THE MAJOR FISCAL ISSUE PREOCCUPYING THE CORTES and the crown during the first half of the seventeenth century centered on the *millones*, the name given to an extraordinary subsidy first conceded by the Cortes in 1590 and renewed periodically from 1601 on. The *millones*, so called because each subsidy was valued at several million ducats annually, gained instant notoriety and remained unpopular for over two centuries.¹⁵ The inequities and economic dislocations that

¹³ This is not an unusual failing. In English history, a detailed critique of the standard "Whig" interpretation of parliamentary history is only now gaining momentum. See, for example, Conrad Russell, "Parliamentary History in Perspective, 1604–29," *History*, 61 (1976): 1–27; Mark Kishlansky, "The Emergence of Adversary Politics in the Long Parliament," *Journal of Modern History*, 49 (1977): 617–40; and J. H. Hexter, "Power Struggle, Parliament, and Liberty in Early Stuart England," *ibid.*, 50 (1978): 1–50.

¹⁴ *Las Actas de las Cortes de Castilla*, 60 vols. (Madrid, 1877–1974). These recorded minutes begin with the session of the Cortes in 1563 and continue to 1656 with some important gaps—namely, the sessions of 1576–78, 1638–43, and 1646–47. The unpublished minutes for the session of 1660–64 are located in the Archivo del Congreso de los Diputados (hereafter, ACD) in three bound volumes in manuscript entitled "Actas de las Cortes de Castilla, 1660–64." Documentation relating to the Cortes of the late medieval period up to 1537 appears in *Cortes de Los Antiguos Reinos de León y de Castilla*, 4 vols. (Madrid, 1883).

¹⁵ Antonio Domínguez Ortiz, *Política y hacienda de Felipe IV* (Madrid, 1960), 237–38, and "La Desigualdad contributiva en Castilla durante el siglo XVII," *Anuario de Historia del Derecho Español*, 21–22 (1951–52): 1222–68; Antonio Matilla Tascón, *La Única contribución y el catastro de la Ensenada* (Madrid, 1947), 19–24; José Luis Sureda Carrión, *La Hacienda Castellana y los Economistas del siglo XVII* (Madrid, 1949), 161–66, 181–85; and Modesto Ulloa, *La Hacienda real de Castilla en el reinado de Felipe II* (2d ed., Madrid, 1977), 519–22.

it produced seriously harmed the Castilian economy, and reformers consistently advocated its repeal and replacement. By granting the *millones* in the first place and by renewing it thereafter, the Cortes did not, however, necessarily throw Castilian interests to the wind and slavishly follow the dictates of the crown. The reality was quite different. As a direct result of the *millones*, the Cortes acquired extensive fiscal and administrative powers and increased its political influence. The eighteen cities it represented gained a jurisdiction that was provincial in scope, and their governing oligarchies secured dazzling opportunities for financial gain. Seen from this perspective, the *millones*, for a brief period, began to alter the balance of political forces within Castile.

This conclusion is inescapable given that the *millones* was not a tax but a tax agreement. The distinction is crucial. A tax is a specific means defined by law and backed by the coercive power of the state for raising revenue; it is a form of official extortion perpetrated by civil authorities and legitimized by appeal to a generally shared conception of the "public good." In contrast, a tax agreement is an accord negotiated by various levels of government for the sharing or transferring of public revenues. In modern federal states, the agreements might involve national, provincial or state, and municipal governments. This modern analogy has severe limitations for clarifying the exact nature of the *millones*, but the comparison is still illuminating.

The first *millones* contract followed fast upon the defeat of the Spanish Armada in 1588. Before this seemingly catastrophic event, Philip II (1556–98) had repeatedly tried to convince either the Cortes or the cities it represented to approve a major new tax only to have his efforts consistently thwarted.¹⁶ The *procuradores* in the Cortes and the *regidores* in their municipal councils did not suddenly abandon their opposition to new tax schemes in the face of this crisis; nevertheless, the damage to Castilian national pride and the perceived threat to national security inclined them toward compromise. After two years of intensive negotiation, they agreed to the introduction in 1590 of a temporary subsidy worth eight million ducats spread over six years.¹⁷ This agreement established a format for the increase of taxation without stipulating exactly what those taxes would be. As a result, it did not fulfill Philip II's lifetime ambition to secure a significant new tax resource.

In the form as agreed upon in 1590, each municipality was apportioned a share of the subsidy, and the choice of what taxes or other fiscal expedients would be used was left to each town and city to arrange individually with the senior members of the royal Council of Castile, the highest judicial tribunal in the realm.¹⁸ Some towns chose to introduce new *sisas*, indirect taxes on basic foodstuffs, or to increase the weight of existing *sisas*; some resorted to capitation taxes; some raised money by leasing out public lands; some licensed taverns or selected from a host of other *medios* and *arbitrios*. In short, the first *millones* con-

¹⁶ Lovett, *Philip II and Mateo Vázquez*, 69–73, 103–04; and Ulloa, *La Hacienda real de Castilla*, 85, 177.

¹⁷ Ulloa, *La Hacienda real de Castilla*, 505–08.

¹⁸ *Ibid.*, 508–09; and *Las Actas de las Cortes de Castilla*, 10: 397–425, and 11: 287–90, 321–31, 472–76.

sisted of a bewildering complex of bilateral accords framed by the guidelines of a general tax agreement—a type of intergovernmental, tax-sharing arrangement involving the Castilian municipalities and the crown. The Cortes acted essentially as the mediator; once it had negotiated and given conditional approval to the original agreement, it played no further role. The cities, by accepting the agreement, offered to exploit their own fiscal and property resources and those of the towns and villages within their provinces for a limited period of time in order to raise the revenues required to rebuild the armada. But, because they controlled the fiscal devices they chose to employ, they in effect did not cede any new tax or any of their own fiscal prerogatives to the crown. Their generosity, sparked by a national calamity, remained tempered by political hard-headedness.

The experiment of raising national revenue from municipally controlled resources proved to be a qualified disaster. The crown, it is true, received much-needed additional funds, but discontent was rife. Local oligarchs were accused of abusing their new fiscal powers; nobles protested when forced to pay capitation taxes; the *Mesta*, the Castilian guild of shepherders, complained over the forced conversion of pasture lands to arable; peasants condemned the loss of commons; and local protests and rebellions broke out.¹⁹ Convinced of the need both to renew and to reform the *millones*, Philip locked himself into a struggle with the Cortes as early as 1593.²⁰ The ensuing debate was unusually embittered; it marks one of those rare instances when the Cortes gained sufficient courage openly to challenge government policies. The most outspoken *procuradores* attributed the advent of recessionary economic conditions to excessive taxation, criticized the growing scale of government expenditures, condemned the scope and cost of Habsburg foreign policy, and expressed apprehension about the adequacy of Spain's own coastal and maritime defences.²¹ So entrenched and unrelenting was this parliamentary opposition to an extension of the subsidy that the Marquis of Poza, president of the Council of Finance, advised Philip on several occasions to dissolve the Cortes and convoke a more amenable one in its stead.²²

The cities, once they became involved in these negotiations, proved equally stubborn and uncooperative. Not until February 1597 did Philip finally wear down the resistance within the Cortes and win assent for an annual subsidy of about 1.3 million ducats—a sum to be raised entirely from *sisas*.²³ But the Cortes did not have the authority to conclude such agreements entirely on its own. The powers provided to the *procuradores* by their cities were limited, sometimes explicitly through the issuance of special instructions and oaths of obedience, some-

¹⁹ Ulloa, *La Hacienda real de Castilla*, 519; and *Las Actas de las Cortes de Castilla*, 12: 95–97, 120, 163–64, 300–07, 413.

²⁰ Ulloa, *La Hacienda real de Castilla*, 530–31; and *Las Actas de las Cortes de Castilla*, 12: 372–81.

²¹ *Las Actas de las Cortes de Castilla*, 12: 444–81, 13: 232–35, 542–97, and 15: 64–99.

²² Marquis of Poza to Don Cristóbal de Mora, British Library [hereafter, BL], Additional MS 28,378, ff. 13–14, 29–32, 198–99.

²³ *Las Actas de las Cortes de Castilla*, 15: 64–99, 428–63.

times implicitly by expectations based upon established custom and practice.²⁴ It was in accordance with these normal procedures that the accord was submitted for final approval to the eighteen cities with votes in the Cortes.²⁵ The cities were reluctant and slow to comply. By April 1598, only a bare majority of ten had endorsed the agreement, and most of these had qualified their support by adding harsh supplementary conditions of their own.²⁶ The situation amounted to a stalemate that persisted up to the moment of Philip II's death on September 13, 1598. When his successor, Philip III, attempted to implement the agreement on the basis of this bare majority, he too was rebuffed. His acknowledgement of the defeat of the tentative *millones* accord of 1597 came in November 1598, when he dissolved the Cortes.²⁷

The loss of income from the *millones* threatened to leave royal finances in a permanent state of collapse, and only through patient and resolute diplomacy did Philip III and his aristocratic court favorite, the Duke of Lerma, resolve this dangerous political crisis. In the process, they established the basis for a new relationship between the Spanish crown and the Cortes and cities of Castile. Between 1598 and 1601 they went to extraordinary lengths to placate, persuade, favor, bribe, and otherwise try to influence first the *procuradores* and then the urban *regidores* to accept the new *millones* agreement. Their sojourn in various cities in Old Castile in 1600 was directly related to this effort.²⁸ Such active politicking paid handsome dividends. After 1601, the *millones* became a permanent feature of Castilian government finances. But its longevity was never assured. The negotiations leading to the agreement of 1601 took almost two years to complete, and those for the ensuing contracts of 1603, 1610, and 1619 were equally protracted.²⁹ In all cases there were pockets of intense resistance, and in none was the unanimous consent of the cities ever achieved.

From the perspective of the crown, the second generation of *millones* contracts, beginning with the subsidy of 1601, represented a significant advance over the *millones* of 1590–96. Revenues no longer flowed from a confusing array of local tax agreements; they now derived entirely from *sisas* applied uniformly through-

²⁴ For the Cortes of 1563, for example, of the fourteen cities on which information is available, eight issued special instructions and, of these, six required oaths; *Las Actas de las Cortes de Castilla*, 1: 14–16, 34. In 1576, twelve of the eighteen cities issued instructions; Manuel Dánvila, "Introducción," *ibid.*, 5 (ii): 19. Lovett, therefore, was quite justified in drawing particular attention to this feature of Castilian parliamentary life; see *Philip II and Mateo Vázquez*, 104. In contrast, John Lynch incorrectly viewed the decision of the Cortes to consult the cities in 1624 as a fundamentally new departure; Lynch, *Spain under the Habsburgs*, 2 (London, 1969): 89.

²⁵ For the correspondence between the *corregidores* and the president of the Council of Castile concerning their efforts to persuade the cities to accept the proposal, see Archivo General de Simancas [hereafter, AGS], Patronato Real, Caja 85.

²⁶ Archivo Municipal de Valladolid [hereafter, AM Valladolid], Libro de Acuerdos, 1597–99, April 17, 1598, ff. 490–91. The *corregidor* reported that a majority of ten cities had now agreed in an effort to persuade the *regidores* to comply.

²⁷ *Las Actas de las Cortes de Castilla*, 15: 710–11. Philip's efforts to persuade the cities to approve this agreement are recorded in the municipal council minutes of Valladolid and Córdoba; see AM Valladolid, Libro de Acuerdos, 1597–99, October 7, 1598, ff. 652–53; and AM Córdoba, Libros Capitulares, 1598, October 5, 1598, f. 325.

²⁸ Luis Cabrera de Córdoba, *Relaciones de las cosas sucedidas en la corte de España desde 1599 hasta 1614* (Madrid, 1857), 69, 71, 78, 84.

²⁹ For the detailed terms of the agreements of 1601, 1603, 1610, and 1619, see *Las Actas de las Cortes de Castilla*, 19: 673–705, 21: 679–716, 24: 181–201, and 32: 448–561.

out the realm. In 1603, when these *sisas* were levied against the "four species" (wine, vinegar, olive oil, and meat), the *millones* acquired what became its classic form.³⁰ But the *millones* was never merely a subsidy; it was also a tax-sharing arrangement. *Sisas* already had a long history in Castile as a municipal tax resource. Although their exact legal status was clouded by the limitation that cities and towns could only impose *sisas* with the specific consent of the crown, they were widely used by municipalities as a means of raising extraordinary revenue.³¹ As a source of taxation, therefore, *sisas* were thoroughly identified with municipal governments.

The successive *millones* agreements from 1601 on, therefore, followed the pattern established in 1590; they continued to involve a transfer of public revenue from one level of government to another. The cities levied *sisas* on behalf of the crown, but they did not cede any of their jurisdictional rights. They retained administrative control over their own *sisas* and, more importantly, extended their traditional jurisdiction by acquiring indirect control over the administration of *sisas* collected by other towns and villages within their provincial spheres of influence. At the national level, the collection of *millones* revenue was centralized through the Cortes. The royal Council of Finance was permitted only to expend this revenue, and then as directed by the terms of a budget attached to the agreement. This budget earmarked monies for maintaining coastal and maritime defenses, for subsidizing the administrative councils of the realm and the courts, and for supplying the royal household.³² The administrative powers acquired by the Cortes and cities and their pretension to supervise expenditures were precisely the concession that Philip II had staunchly resisted until the final years of his reign, when it became manifestly clear that no subsidy would be granted without them.³³

The *millones*, therefore, gave rise to a public system of tax administration distinct and separate from that under the jurisdiction of the Council of Finance.³⁴ The system was hierarchical. Regional centers (*cabezas de partido*) collected their own *sisas* and those of dependent villages. The eighteen cities with votes in the Cortes did likewise; but they gained the additional rights to investigate the ad-

³⁰ *Ibid.*, 20: 695, and 21: 6-30, 62-64.

³¹ J. A. Bonachía, *El Concejo de Burgos en la baja edad media (1345-1426)* (Valladolid, 1978), 138-40; M. González Jiménez, *El Concejo de Carmona a fines de la Edad Media (1464-1523)* (Seville, 1973), 231; Sureda Carrión, *La Hacienda Castellana y los Economistas del siglo XVII*, 152-56; and M. A. Ladero Quesada, *Historia de Sevilla, La Ciudad Medieval* (Seville, 1976), 150. There are abundant references to the use of *sisas* in municipal archives; one list of *sisas* in Córdoba provides detail on the reasons for their introduction at various times in the past. See AM Córdoba, *Libros Capitulares*, 1609, f. 128.

³² *Las Actas de las Cortes de Castilla*, 19: 335. The revenues from the *millones* was initially intended to be used to discharge debts; hence, the budgets were to apply to an equivalent amount of revenue from other taxes. In I. A. A. Thompson's opinion, the military appropriations prescribed were based on "incomplete" and "ill-founded" cost estimates; see *War and Government in Habsburg Spain*, 78.

³³ *Las Actas de las Cortes de Castilla*, 15: 385-86.

³⁴ On June 6, 1618, in the course of a review of the previous conditions attached to *millones* agreements, a committee of the Cortes reported that "the principal conditions agreed to when the quantity of eighteen million was tentatively conceded [in 1600] . . . was that the Cortes be the sole administrator and distributor of this subsidy"; *Las Actas de las Cortes de Castilla*, 31: 600-01. For an extended discussion of this condition and its implications, see Francisco Gallardo Fernández, *Origen, progresos, y estado de las rentas de la Corona de España Su Gobierno y Administración*, 1 (Madrid, 1805): 47-66.

ministrative activities of the regional centers, to adjudicate local disputes, to set up central depositories and agglomerate all of the revenues collected within their provinces, and to control the release of funds. These new powers enhanced the authority and the prestige of those eighteen cities with parliamentary representation and buttressed their ambitions to become true provincial capitals. Their *regidores* also acquired new sources of profit and influence. These arrangements, so highly beneficial to a few cities, not surprisingly rankled other municipalities, some of which actively resisted this new form of subordination.³⁵ Their resentment helps explain the growing demand for representation in the Cortes.³⁶

At the apex of the *millones* structure stood the Cortes; it audited the accounts submitted by the cities and ensured that the subsidy was paid as agreed. It did not, however, serve as a court of final appeal; this function was settled on the Council of Castile. Within this public system of tax administration, an agency called the Commission of *Millones* assumed a role roughly parallel to that of the Council of Finance within the traditional fiscal system. Created in 1602, when the Cortes recognized the need to delegate routine decisions, the commission consisted of four *procuradores* elected for staggered six-month terms.³⁷ It became a permanent body with a secretariat in 1611, when for the first time its life was extended to cover periods of parliamentary recess.³⁸ By the 1640s, the commission was managing more tax revenue than the Council of Finance.³⁹

Although the Cortes and cities effectively asserted their claim to administer the *millones*, they were less successful in influencing expenditures. Their detailed budgets were ignored, and their expectation that the revenue would be used to pay off public debts was consistently disappointed. Finally, in 1619, outrage over the diversion of funds to foreign wars and anger over Spain's continued vulnerability to corsair raids spurred the *procuradores* to insert into the *millones* contract a clause requiring the crown to apply to the Cortes or, in its absence, the Commission of *Millones* for the release of funds, specifying the purpose for which the monies were intended.⁴⁰ Moreover, the new condition required that royal officials submit accounts to prove that the funds had been spent in the manner approved. These new regulations made unequivocal the distinction that had now crystalized between public income under the jurisdiction of the Cortes and royal income administered separately by the Council of Finance.

³⁵ ACD, Libros de la Comisión de Millones, Libro 3, September 10, October 8, and October 13, 1615. On these occasions, the minutes refer to disputes involving Toro and Palencia, various cities in Extramadura and Salamanca and Zamora and Santiago.

³⁶ Domínguez Ortiz has attributed this desire to the lucrative benefits to be derived from the office of *procurador*, but this explanation is insufficient. The three areas granted representation by Philip IV—Galicia (1622), Palencia (1650), and Extramadura (1650)—had all resisted the administrative authority held over them by Zamora, Toro, and Salamanca in matters pertaining to the *millones*. See Ortiz, "Concesiones de Votos en Cortes a Ciudades Castellanas en el Siglo XVII," *Crisis y decadencia de los Austrias* (Madrid, 1969), 99–111.

³⁷ *Las Actas de las Cortes de Castilla*, 20: 92.

³⁸ *Ibid.*, 26: 138. The records of the commission exist from this time; see ACD, Libros de la Comisión de Millones, 5 vols.

³⁹ Joseph González, the president of the Council of Finance, reported to Philip IV in 1648 that "Your Majesty's principal sources of income, surpassing six million ducats, are administered by this commission"; AGS, Consejo y Junta [hereafter, CJH], legajo 951, Consulta del Presidente, September 2, 1648.

⁴⁰ *Las Actas de las Cortes de Castilla*, 31: 609–10, and 35: 151–65.

BY THE TIME PHILIP IV ASCENDED THE THRONE in 1621, the monarchy was in the process of losing a significant measure of financial autonomy: it lacked direct administrative control over the *millones* and now found itself forced to consult the Cortes on expenditures. The seriousness of these limitations is apparent in the detailed government budgets for the period 1619–21. They reveal that, on average, 30 percent of the revenue available annually to finance current expenditures derived from the *millones*.⁴¹ The problems encountered by royal officials in the use of these funds placed them in what many regarded as an untenable position. On the one side, they were barraged by complaints from government financiers who, angered by chronic delays in the payments assigned to them from the municipally controlled *millones*' revenue, threatened to suspend credit transfers abroad. On the other, every time they tried to intervene in the administration of the *millones*, they met fierce opposition from the Cortes. They could not afford to antagonize the *procuradores*, especially since the more militant were already advocating the total withholding of *millones*' payments pending the receipt of accounts for previously approved expenditures.⁴² Obviously, the *millones* agreement of 1619 had given the Cortes a weapon with the potential to paralyze the conduct of government.

These difficulties were symptomatic of a serious weakness afflicting the Castilian monarchy. No matter where it applied or how it was given, public consent for taxation could ultimately limit the scope and independence of government. This was certainly true in the England of the Stuart kings; it was equally true of other states where parliaments flourished.⁴³ In Castile, the Habsburgs were exposed to a kind of double jeopardy; the introduction of new taxes and the renewal of existing subsidies not only required the consent of the Cortes but also of a majority of those cities directly represented by it. As both Philip II and Philip III painfully discovered, the approval of the cities was often difficult to achieve. The existence of this criterion provided the urban patricians with a major trump in their dealings with the crown. By playing this trump, they tightened their administrative control over the *millones*, imposed more rigorous restrictions on expenditures, and increased and systematized the numerous legislative conditions attached to the tax agreement.⁴⁴

But these gains were still not sufficient to sate the appetites of Castile's most radical reformers. Their most outstanding spokesman during the early years of Philip IV's reign was Don Mateo Lison y Biedma, a *regidor* in Granada and that

⁴¹ The proportion of income from the *millones* to total tax income available for current expenditures was 27.7 percent in 1619, 36.8 percent in 1620, and 28.9 percent in 1621. See AGS, CJH, legajo 561, no title, no date, legajo 567, Consulta, January 23, 1620, and legajo 573, Consulta, December 10, 1621.

⁴² AGS, CJH, legajo 555, Consulta, March 18, 1618, and legajo 602, no. 224, Consulta, November 7, 1623. For the position of the Cortes in these disputes, see ACD, Libros de la Comisión de Millones, Libro 3: March 6, 1616, September 13, 1619, and Libro 4: January 14, 1622, August 22; and *Las Actas de las Cortes de Castilla*, 32: 56–57, 34: 425–26, and 35: 77–79.

⁴³ For a general survey of the history of European parliaments during the late medieval and early modern periods, see A. R. Myers, *Parliaments and Estates in Europe to 1789* (London, 1975).

⁴⁴ *Las Actas de las Cortes de Castilla*, 32: 448–561. In 1619, these conditions were divided according to five general categories: regulations pertaining to *sisas*, administrative arrangements for the *millones*, stipulated expenditures, legislation controlling the *Mesta*, and, finally, miscellaneous terms and conditions.

city's *procurador* to the Cortes in 1621. Lison y Biedma was hostile to increased taxation, demanded government spending restraints, advocated municipal control over public revenues, and favored the adoption of a formal system of consultations between the crown and the municipalities on such matters as senior judicial appointments.⁴⁵ As a reformer, he was unusually prolific and outspoken, but he was no maverick and appears to have enjoyed widespread support. In 1624, when he publicly denounced the renewed minting of copper currency and tried to organize municipal opposition, the president of the Council of Castile urged Philip to reject the advice proffered by the Council of Finance to have Lison y Biedma arrested; the president feared that such an action would turn the reformer into a martyr and further stir popular discontent.⁴⁶ The anti-government attitudes that Lison y Biedma reflected manifested themselves in a less dramatic way about the same time. In their negotiations with the crown over the introduction of a national banking system, a reform that had been under sporadic consideration since the 1570s, the cities were now insisting that the banks serve as depositories for all tax revenues and that they effectively be under municipal control.⁴⁷

These reforms were the logical extension of the regime of the *millones*. Therefore, it is not surprising either that their most militant advocate assumed such an overt and unabashed pro-urban stance or that the most vigorous opposition to this reform movement came from the government. Here, the dominant figure was the Count-Duke of Olivares. He also championed reform, but his reforming ideas were linked to a rigid and uncompromising concept of absolutism.⁴⁸ But Olivares was motivated more by harsh political realities than by abstract theories of government. By the early 1620s, Spain was inextricably caught in the diplomatic and military web of the Thirty Years' War. Circumstances necessitated an active and aggressive foreign policy, which in turn required a healthy treasury at a time when Castile was drifting relentlessly toward bankruptcy. In this extremely dangerous situation, Olivares clearly considered intolerable the government's lack of fiscal autonomy and incomplete control over a significant portion of public revenue.

For reformers on both sides of this ideological divide, taxation was the crucial issue. Lison y Biedma emphasized the need for consent and drew attention to the moral duty of *procuradores* and *regidores* to ensure that royal taxation did not exceed the capacity of the realm to pay.⁴⁹ In contrast, Pedro Fernández Navarrete, in his *Conservación de Monarquías y Discursos Políticos* (a publication that became a virtual handbook for the official, government-sponsored reform move-

⁴⁵ Jean Vilar, "Formes et tendances de l'opposition sous Olivares: Lison y Viedma, defensor de la patria," *Mélanges de la casa de Velázquez*, 7 (1971): 263-94.

⁴⁶ AGS, Secretaría de Gracia y Justicia, legajo 878, Consulta, Presidente del Consejo Real, July 6, 1626.

⁴⁷ Ruiz Martín, "La Banca de España," 61-74, 94-95.

⁴⁸ The best biography of Olivares remains Gregorio Marañón, *El Conde-Duque de Olivares: La Pasión de Mandar* (Madrid, 1936). For Olivares's ideas, see J. H. Elliott and José F. de la Peña, *Memoriales y cartas del Conde-Duque de Olivares*, volume 1: *Política interior, 1621-1627* (Madrid, 1978).

⁴⁹ Lison y Biedma, "Desengaños de Rey y Apuntamientos para su Gobierno . . .," BL, Additional MS 9,935, ff. 37-105, and "Discursos y Plática entre cierto ministro favorecido y un veintiquatro sobre la concesión de Millones," *ibid.*, 106-13.

ment), expressed the opposite view. Although he regarded taxes as a necessary evil, comparing them to the shedding of tears and blood, he gave more weight to their advantages than to their disadvantages. Recognizing the "benefits of peace and tranquility" that they enjoyed, Castilians, he argued, should acknowledge that "grave illnesses can only be cured by copious bleedings and there can be no peace without arms, no arms without money, and no money without taxes." He also dispensed with the need for public consent. In difficult times, he wrote, subjects should serve their king with taxes without awaiting "the tardy resolutions of the Cortes." To illustrate his point, he noted that, "when a pilot sails in fair weather, he does not throw overboard the merchandise . . . in his care but, when storms oblige him to lighten his ship, he does not await the consent of the owners to eject even the most precious luxuries."⁵⁰ A number of other writers shared this opinion, although some, including Saavedra Fajardo, emphasized the advisability, rather than the necessity, of securing parliamentary consent for extraordinary taxes.⁵¹

This debate coincided with a growing revulsion toward the *millones*—a revulsion that royal officials cleverly exploited to justify the repeal of a tax system increasingly unfavorable to the overall interests of the crown. Complaints against the *millones* were legion. *Sisas* were blamed for driving up the cost of living, and, since they fell upon consumers rather than producers and weighed more heavily upon the poor than the rich, they were condemned for being grossly inequitable as well. Municipal officials were also accused of blatantly engaging in speculation and of using their administrative powers over the *millones* and their political influence to reap personal profit. The Count-Duke of Olivares joined in this chorus of criticism; what the beleaguered peasants and artisans contributed to the king out of devotion and loyalty, he claimed, these wealthy parasites of the public purse diverted to their own use.⁵² Given this climate of opinion, government officials could easily rationalize Philip IV's desire to destroy once and for all the regime of the *millones*.

Philip launched his first assault against the *millones* between 1622 and 1625. In 1622, on the basis of recommendations advanced by the *Junta de Reформación*, an ad hoc government committee with decidedly pro-absolutist leanings, Philip proposed that the *millones* be replaced by a financial commitment from the cities to maintain a permanent standing army of thirty thousand men. Furthermore, he supported the introduction of a national banking system but only one that was firmly under royal jurisdiction. These banks were to be initially capitalized by the temporary imposition of a 5 percent wealth tax. These measures were not as shocking or as novel as were the unorthodox means by which Philip sought to introduce them. First, he bypassed the Cortes and submitted his plan directly to the cities, thereby eliminating the first formal stage in normal consultative and

⁵⁰ Fernández Navarrete, *Conservación de monarquías y discursos políticos* (Madrid, 1805), 111.

⁵¹ Fajardo, *Idea de un príncipe político cristiano*, ed. Vicente García Diego, 3 (Madrid, 1927): 179.

⁵² Archivo Histórico Nacional [hereafter, AHN], Estado, libro 853, ff. 1–68, Consulta, September 22, 1634. Olivares had indicated his contempt for *regidores* in earlier writings as well; see Olivares, in Elliott and de la Peña, *Memoriales y cartas*, 64–65, 249–50.

approval procedures.⁵³ Only when the cities refused to comply, defending in the process the right of the Cortes to deliberate on such matters, did Philip finally issue parliamentary writs.

Once convened, the *procuradores* also rejected the terms of the original proposal, although they were willing to compromise. They approved an annual subsidy of 4.9 million ducats, more than twice the value of the existing *millones*, with the additional sum to be raised by taxing the wealth of rentiers, office-holders, and propertied persons.⁵⁴ The accord was referred to the cities in February 1624, and this time Philip left nothing to chance. Whether consciously or unconsciously, he imitated the travels undertaken by his predecessor in 1600, except that, whereas Philip III had headed north into an area where the Duke of Lerma held influence, Philip IV headed south into the bailiwick of the Count-Duke of Olivares. His visit to the cities of Córdoba, Seville, Granada, Málaga, and Jaén was timed to coincide with debates in municipal councils over the new tax proposals.⁵⁵ The effort to persuade was carefully coordinated; the speech delivered to the assembled dignitaries of Valladolid by its *corregidor* was identical to that presented by Olivares in Seville.⁵⁶ Nevertheless, the tactics failed; once again, a majority of the cities refused to comply. By August, senior government officials were debating the drastic measure of introducing the new subsidy without majority approval. "This is a business that makes great noise here," reported the English ambassador, "as attempting to make so great an alteration contrary to what hath been long here the custom and practice of these parliaments."⁵⁷ In the end, prudence prevailed. On September 19, Philip officially terminated his efforts to gain municipal consent when he instructed the Cortes to begin again to review tax proposals in order to devise another, alternative subsidy arrangement.⁵⁸

The terms of the substitute agreement were finally approved by the Cortes on May 1, 1625—more than three years after the original proposals had been referred to the cities—and subsequently approved by a majority of the urban councils. The accord renewed the *millones* of 1619 but doubled its value by tacking on an additional *alcabala*, or general sales tax, of 1 percent. In the course of its deliberations, the Cortes also agreed to excise from the *millones* contract the severe restrictions imposed in 1619 on the expenditure of funds.⁵⁹ Thus Philip IV gained more revenue and restored to his government total control over the

⁵³ Angel González Palencia, "La Junta de reformatión," in *Archivo Histórico Español*, 5 (Madrid, 1932): 398–408; and Ruiz Martín, "La Banca de España," 74–94.

⁵⁴ *Las Actas de las Cortes de Castilla*, 40: 241–47. The additional taxes included a 5 percent levy against bond and mortgage income, a 12 percent tax on the income from public offices, a 20 percent tax on pensions, a 1 percent tax on the produce of all lands and properties subject to the tithe, and indirect taxes on various luxuries, on salt, and on the services of money-changers and scribes.

⁵⁵ "Carta escrita de un criado del Rey de los que ban en la jornada," Seville, March 9, 1624, Bodleian Library, Arch Z 141.

⁵⁶ AM Seville, Libros Capitulares, 1624–25, March 5, 1624; and AM Valladolid, Libro de los Ayuntamientos, 1623–25, March 11, 1624.

⁵⁷ Aston to Secretary Edward Conway, August 8, 1624 (OS), BL, Additional MS 33, 499, f. 135. I am grateful to J. H. Elliott for this reference.

⁵⁸ *Las Actas de las Cortes de Castilla*, 41: 442–46.

⁵⁹ *Ibid.*, 62: 379–80, and 41: 140–41, 150–55, 167–70, 180–84. Instead, the crown was to be given a blanket release of funds year by year and in return provide detailed accounts of expenditures. There is no evidence that such accounts were ever submitted.

disbursement of public funds. These gains were not insignificant, but they were still less than what he had sought. Castile's urban elite had defeated his attempt to introduce wealth taxes and thereby create a more equitable and remunerative system of taxation. In addition, they had retained intact the basic elements of the regime of the *millones*, despite his government's concerted effort to destroy it.

Philip's most desperate bid to defeat the *millones* came in the wake of the bankruptcy of 1627. In January 1631 he announced his unilateral decision to repeal the *millones* and to introduce a French-style *gabelle* to replace it. No public discussion preceded this action, nor did Philip seek the consent of the Cortes; he justified such action by claiming that salt taxes were part of the king's regalia. The enabling legislation contained a lengthy critique of *sisas* and of the inequitable manner in which they were administered.⁶⁰ In comparison, the salt tax was portrayed as an equitable means of taxing both rich and poor. The arguments were more convincing in theory than in practice. Once the price of salt was jacked up to prohibitive levels and exorbitant quotas assigned, a militant opposition quickly emerged. There was a rebellion in Vizcaya, a serious confrontation with the church in Seville, and strained relations with urban officials throughout Castile.⁶¹

The difficulties encountered in implementing the *gabelle*, an impending shortfall in revenue resulting from the repeal of the *millones*, and the crown's desperate need for additional revenue convinced Philip to convoke the Cortes in February 1632 in order to seek an extraordinary subsidy of nine million ducats over three years. The *procuradores* categorically rejected the request, defended the *millones*, and demanded both the repeal of the *gabelle* and a commitment never to reintroduce it. After obtaining these concessions, they approved the requested subsidy and renewed the *millones*.⁶² Once again, the *procuradores* of the Cortes had successfully defended the regime of the *millones*.

THE VICTORY OF THE CORTES OF 1632 nevertheless marks a constitutional turning point in the history of Habsburg Castile. When Philip convoked the Cortes, he insisted that the cities grant their *procuradores* full powers, thus enabling them to conclude agreements entirely on their own. He warned them that he would refuse to accept the credentials of representatives bound by special instructions and oaths of obedience. The text prescribed for the issuance of powers appears to have been rejected by the cities: it was so explicit that it was regarded as unacceptable. Even so, the cities also appear to have refrained from imposing special instructions and oaths on their delegates.⁶³

⁶⁰ Domínguez Ortiz, *Política y hacienda de Felipe IV*, 235. For documentation pertaining to the salt tax, see AGS, Contraduría Mayor de Cuentas, 3ª época, legajo 1, 210. For the rebuttal of the Cortes, see *Las Actas de las Cortes de Castilla*, 49: 90–100.

⁶¹ Domínguez Ortiz, *Política y hacienda de Felipe IV*, 265; and Quintín Aldea, *Iglesia y estado en la España del siglo XVII* (Comillas, Santander, 1961), 38.

⁶² *Las Actas de las Cortes de Castilla*, 49: 175–92, 334–66.

⁶³ AM Burgos, Libros Capitulares, 1632, ff. 20–28; AM Granada, Libros de Actas de Sesiones, 11: ff. 78–80; AM Córdoba, Libros Capitulares, 1632, ff. 31–33; and AM Toledo, Cortes, 1601–1655, Escritura de Poder, 1632.

Once the Cortes was convened, Philip, through his officials, pressured the *procuradores* into hastily formulating a new subsidy agreement and demanded that they give it unconditional and final approval. Some refused; other balked, uncertain that their delegated authority was sufficient to allow them to consent to new subsidies on their own. To remove this uncertainty, Philip referred the question to the Council of Castile, where the supreme judges of the realm ruled in favor of the crown's interpretation of the *procuradores'* traditional powers.⁶⁴ Following this decision, the crown publicly campaigned to win general support for its view that the *procuradores* were the plenipotentiaries of their cities and could not only negotiate but also make binding tax agreements. Among those who championed the opposite opinion was a leading literary figure of the period, Don Francisco de Quevedo.⁶⁵ Despite this debate, the crown for the first time managed to get approval for and implement a major new subsidy without the prior consent of the cities that had representation in the Cortes.

This fundamental change in standard parliamentary practices had immediate consequences. Exposed to the vast array of tantalizing inducements that the government could command, unrestrained by the fear of having their generosity repudiated by the more parsimonious cities, and preoccupied first by the imminence and then by the outbreak of war with France, the *procuradores* loosened the purse strings of the realm throughout the 1630s and early 1640s. They approved additional subsidies to supplement what now became known as the "ancient *millones*" and even deigned to concede taxes directed against the rich, such as a graduated hearth tax in 1640 and a 2 percent tax on property income in 1642.⁶⁶ A more isolated and submissive Cortes also facilitated a gradual government takeover of the *millones*. In July 1632, a majority of the *procuradores* acquiesced to Philip's request for permission to appoint to the Commission of *Millones* four senior royal officials to share the administrative responsibilities with the four representatives chosen from the Cortes. Shortly thereafter, the crown also secured the right to appoint administrators to supervise the collection of revenues in cities and towns where arrears on the *millones* were chronic.⁶⁷ In this fashion, royal officials began to infiltrate the administrative apparatus of the *millones* from both the top and the bottom.

The cities did not accept these changes without protest. Their discontent surfaced every time they were required to appoint new *procuradores* with unlimited powers. These occasions gave the urban oligarchs an opportunity to vent their spleens and voice their anger and frustration. They were particularly outraged by the dramatic increase in taxation from which they were losing their immunity and by the government's policy of withholding interest payments on

⁶⁴ *Las Actas de las Cortes de Castilla*, 49: 134–35, 209–10, 218–20, 225–33, 235–51, 263–69.

⁶⁵ José Antonio Maravall, "El Tema de las Cortes en Quevedo," in *Estudios de historia del pensamiento Español: Siglo XVII* (Madrid, 1975), 352. For the royalist position, see Antonio Hurtado de Mendoza, *Convocación de las Cortes de Castilla y juramento del príncipe . . . don Baltasar Carlos* (Madrid, 1632).

⁶⁶ AGS, CJH, legajo 806, no. 368, and legajo 555, Consulta, September 3, 1642. For a striking illustration of the dramatic increase in the incidence of taxation at this time, see Carla Rahn Phillips, *Ciudad Real, 1500–1750: Growth, Crisis, and Readjustment in the Spanish Economy* (Cambridge, Mass., 1979), 82.

⁶⁷ *Las Actas de las Cortes de Castilla*, 50: 248–51, and 53: 121–24.

bonds—a practice that became regular from the mid-1630s on. In these circumstances some *regidores* were inclined to attribute all contemporary ills—the battered state of the Castilian economy, the revolts in Catalonia and Portugal, the defeats suffered by Spanish armies abroad—to their loss of influence over taxation.⁶⁸ Enough *regidores* refused to delegate full powers to their *procuradores* in 1646 to force an embarrassing delay in the opening session of the Cortes.⁶⁹ In 1655, the crown once again entered into this debate by commissioning several tracts to explain and justify its contention that the *procuradores* were legitimately the plenipotentiaries of their cities.⁷⁰

Opposition to other forms of royal interference in the administration of the *millones* also intensified during this period and reached new highs in 1647. Philip contravened the terms of successive *millones* agreements by annexing the Commission of *Millones* to the Council of Finance on March 4, 1647. Several cities challenged the legality of this decree in the Council of Castile.⁷¹ Following the bankruptcy of October 31, 1647, the president of the Council of Finance, Joseph González, introduced an equally radical departure from time-honored practices. He appointed superintendents of finance to take over the administration of *sisas* and *alcabalas* from municipal officials and centralized local tax depositories under their authority. These reforms provoked strong protests, especially from Seville, at which they were particularly aimed.⁷²

This elite discontent coincided with the sporadic outbreak of popular rebellion throughout the south of Spain between 1647 and 1652.⁷³ The most serious uprisings occurred in Seville, Córdoba, and Granada—cities that were plagued by constant unrest and that were centers of the fiercest opposition to the superintendents of finance. But the members of the urban patriciate do not seem to have participated actively in these rebellions; rather, they were usually pre-eminent in restoring law and order, but they sought and received rewards for their services. To pacify the rebellious and starving masses, royal officials usually removed for a short time indirect taxes on basic foodstuffs; by way of contrast, to appease the rich and the influential, officials made more substantial and less temporary concessions. In October 1647, Philip restored full interest payments on bonds and rescinded the 2 percent tax on income from property. In 1648, he abided by the judgment of the Council of Castile, which declared illegal his attempt to merge the Commission of *Millones* with the Council of Finance. And, in 1650, he responded to continued complaints about the superintendents of fi-

⁶⁸ AM Granada, Libros de Actas de Sesiones, 16: ff. 8–10.

⁶⁹ *Las Actas de las Cortes de Castilla*, 56: 358.

⁷⁰ P. Diego Tello, *Manifiesto por la justicia de Su Magestad* . . . (Granada, 1655). Two other untitled pamphlets appeared at about the same time: the first, anonymous, was published in Seville in 1655; the other, by D. Gregorio Antonio de Chaves y Mendoza, was directed to the city of Córdoba. These three pamphlets are located in the Boornos Archive, libros 47, 52, and 38. I wish to thank the Count and Countess of Boornos for allowing me access to their private manuscript collection.

⁷¹ Gallardo Fernandez, *Origen, progresos, y estado de las rentas de la Corona de España*, 61; and AGS, CJH, legajo 951, Consulta de Joseph González, September 2, 1648. One of the plaintiffs was the city of Toledo; see AM Toledo, Cartas y Varios, 1650, carta de Alfonso Martínez, September 1, 1650.

⁷² AGS, CJH, legajo 920, no. 182, Consulta, December 2, 1647.

⁷³ Domínguez Ortiz, *Alteraciones Andaluas*.

nance by agreeing to withdraw them from all but six cities.⁷⁴ But he continued to move, whenever possible, in the direction of greater royal control over taxation. Philip reintroduced the *media anata* on bonds in 1649.⁷⁵ Through a series of ordinances issued during the 1650s, he wrested the *millones* from municipal control.⁷⁶ And then, in 1658, he finally succeeded in annexing the Commission of *Millones* to the Council of Finance.⁷⁷ Eleven years after his first of several concessions, only the exemption from the property tax remained.

Philip thus partially recovered from the setbacks he experienced during the turmoil of the late 1640s; but his victories were incomplete. Landed property, for example, relieved of the 2 percent tax on rental income, henceforward retained its tax exemption, and wealth taxes never reappeared. Clearly, by the 1650s Philip had lost the momentum to institute significant fiscal reforms. He still yearned for the introduction of a more equitable and remunerative tax system and spoke earnestly in favor of a "medio universal"—a single tax to replace the plethora of indirect taxes burdening Castile. Nevertheless, he repeatedly failed to convince either the Cortes or the cities seriously to consider any of the numerous proposals that his officials advanced.⁷⁸

Even Philip's ultimate success in extending the jurisdiction of his government to cover the administration of the *millones* came too late. What the urban *regidores* had formerly controlled and benefited from directly, they now benefited from indirectly, as bondholders. From 1625 on, the *sisas* linked to the *millones* had, with the consent of the Cortes and the cities, been steadily encumbered by long-term debts (*juros*). As a consequence, the *millones* gradually became integrated into an elaborate and economically destructive tax system that accomplished little more than the redistribution of income from the poor to the rich. Among the greatest beneficiaries of this highly regressive system was the urban elite; its members numbered among the largest bondholders in the realm.⁷⁹ The assignment of bonds to the *millones* also had the effect of perpetuating the subsidy. Consent for renewal was now routinely given—not by the Cortes but by the cities on their own.

What emerged during the 1650s and 1660s was a new form of political stalemate. With the alienation of the *millones*, royal income steadily declined, and the

⁷⁴ AGS, CJH, legajo 907, Consulta de la Junta . . . en la posada del Presidente de Hacienda, September 27, 1647, and legajo 951, Consulta de Joseph González, September 2, 1648; and AM Valladolid, Libros de Acuerdos, 1648–50, November 7, 1650.

⁷⁵ AGS, CJH, legajo 946, Consulta, February 9, 1649.

⁷⁶ In Valladolid, the municipal *comisión de millones* ceased to function sometime during 1645; see AM Valladolid, Libro de Acuerdos, 1644–47, January 3, 1646. In April 1650, the crown appointed a provincial treasurer for the *millones*; *ibid.*, 1648–52, April 6, 1650. The appointment does not seem to have taken effect. More stringent regulations relating to the submission of accounts were issued in 1654; *ibid.*, 1653–55, February 6, 1654. In Córdoba the council debated in 1655 petitioning the crown for the right to compound for the *millones* as a means of regaining administrative control; AM Córdoba, Libros Capitulares, 1655, ff. 302–34.

⁷⁷ Juan de la Ripia, *Práctica de administración y cobranza de las rentas reales* (Madrid, 1676), 190.

⁷⁸ Manuel Dánvila y Collado, *El Poder civil en España*, 5 (Madrid, 1885): 327–30. Philip also expressed his desire for tax reform in his confessional letters to Sor María de Agreda; Francisco Silvela, ed., *Cartas de la venerable Sor María de Agreda y del señor Rey Don Felipe IV*, 2 vols. (Madrid, 1885), 1: 320–33, and 2: 41–42, 160.

⁷⁹ Bartolomé Bennassar, *Valladolid au siècle d'or: Une Ville de Castille et sa campagne au XVI^e siècle* (Paris, 1967), 257; and Phillips, *Ciudad Real, 1500–1750*, 108–09.

crown therefore desperately needed new sources of revenue. Royal officials generally considered counterproductive any attempt to raise additional funds by heaping more indirect taxes on consumer commodities. They realized that as indirect taxes grew, contraband proliferated and yields declined.⁸⁰ Many favored introducing a “medio universal”; included in the proposals they actively considered were land taxes, capitation taxes, luxury taxes, and a tax on flour.⁸¹ Because it proved impossible to gain consent for any of these, Philip was forced to augment his income by raising the already crushing burden of indirect taxes on essential commodities.⁸² The *regidores*, of course, had sound personal reasons for not favoring tax reform. As bondholders with an income derived from the proceeds of existing taxes, they had much to fear from a repeal of *alcabalas* and *sisas*; as men of substance and landed wealth, they had much to lose from a redistribution of taxation. Their intransigent opposition to reform left Philip with only one option: to work within the regime of the *millones*.

Unable to abolish the *millones*, Philip sought to modify it to his advantage. In terms of the taxes themselves, his most important reforms came in 1655 and 1656, when he consolidated and simplified the multiplicity of *sisas* to make them less confusing, less onerous, and easier to administer. More importantly, the burden of payment was shifted from the consumers to the producers.⁸³ These reforms were intended to make the *sisas* more equitable and to reduce opportunities for contraband and fraud. Since they immediately provoked the ire of some of the wealthiest landowners in Andalusia, the reforms appear to have achieved some initial success.⁸⁴ But, however effective they might have been, they were merely palliatives. They may have made the *millones* more tolerable, but they did not fundamentally alter a generally unpopular and discredited system of taxation. Therefore, despite Philip IV's considerable efforts to introduce sweeping tax reforms, the *millones* survived his reign and remained intact to await the later assaults of the “enlightened reformers” of eighteenth-century Spain.

AFTER THE SESSION of 1663–65, the Cortes disappeared from the center of Castilian political life. It no longer served any useful function. It possessed neither the will nor the ability to enact tax reforms and its consent for the renewal of existing subsidies had become virtually redundant. Some ministers regarded it as an inconvenient, wasteful extravagance and advised Philip to dispense with it altogether.⁸⁵ With the advent of Charles II's minority in 1665, this advice was taken. The Cortes receded into the background of Castilian political life to be

⁸⁰ AGS, CJH, legajo 894, Consulta, September 6, 1646; and Boornos Archive, libro 69, Consulta de la Junta de Asistentes, December 18, 1655.

⁸¹ *Las Actas de las Cortes de Castilla*, 59: 33–34, 863–66, 965.

⁸² Domínguez Ortiz, *Política y hacienda de Felipe IV*, 236–38.

⁸³ *Las Actas de las Cortes de Castilla*, 59: 496–510, 624–35.

⁸⁴ Among those hostile to these reforms were members of the aristocracy in southern Spain, in particular the Dukes of Cardona and Medinaceli; see Jago, “The ‘Crisis of the Aristocracy’ in Seventeenth-Century Castile,” 85–86.

⁸⁵ BL, Additional MS 9,936, ff. 206–15.

convened only on ceremonial occasions. In the histories of Habsburg Spain, the passing of the Castilian Cortes scarcely warrants an obituary; it deserves as much.

For over a century, the internal politics of Castile was dominated by the efforts of the crown to increase taxation and by the determination of the urban oligarchs either to resist those efforts or to make concessions only on their own terms. In this contest, the Cortes served as the essential "point of contact" between the crown and the regions of Castile. It managed, often with difficulty, to reconcile the ambitions of a chronically indebted monarchy consumed by dynastic self-interest and those of narrow urban oligarchs intent on preserving and enhancing their wealth, prestige, and power. This continuous struggle brought a creative tension to Castile: public revenues grew, the cities extended their jurisdiction and influence, and the awesome power of Imperial Spain was maintained. Paradoxically, the absolutism that gradually triumphed after 1632 as the Cortes, isolated from the cities, steadily declined in importance and finally disappeared as a meaningful institution, was an absolutism devoid of the potential to act as a dynamic political force. Although he could easily overawe the Cortes and introduce new taxes without municipal consent, Philip IV was still unable to implement major fiscal reforms. The latter years of his reign, particularly after the widespread unrest of the late 1640s, ushered Castile into a prolonged period of political atrophy. Was this the price Castile ultimately paid for resolving, so relatively painlessly, the divisive political and constitutional issues that completely undermined the stability of other seventeenth-century Western European states?

AHR Forum

The Two Postwar Eras and the Conditions for Stability in Twentieth-Century Western Europe

CHARLES S. MAIER

BROADCASTING over the BBC in November 1945, A. J. P. Taylor assured his listeners, "Nobody in Europe believes in the American way of life—that is, in private enterprise; or rather those who believe in it are a defeated party and a party which seems to have no more future than the Jacobites in England after 1688."¹ Taylor proved to be wrong, or at least premature, about the end of private enterprise. The question here is why, at least in Western Europe, there was less transformation than he envisaged. Posed in broader terms, how did Western Europe achieve political and social stability by the mid-twentieth century after two great, destructive wars and the intervening upheaval.

Historians often treat stability as a passive coming to rest or a societal inertia that requires no explanation. In fact, stabilization is as challenging a historical problem as revolution. It can emerge dramatically. As one historian who has focused on the process wrote, "Political stability, when it comes, often happens to a society quite quickly, as suddenly as water becomes ice."² Stabilization, moreover, does not preclude significant social and political change but often requires it. Certainly the two world wars broadened democracy in Britain and stimulated economic transformation in France. World War II finally removed the contradictions between modernity and reaction in Germany, thereby facilitating a meritocratic pluralism. Yet, despite the transformations, earlier liberal and elitist arrangements that governed the distribution of wealth and power either persisted or were resumed after authoritarian intervals. And at least until

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¹ Taylor, "The European Revolution," *Listener* (London), November 22, 1945, p. 576.

² J. H. Plumb, *The Origins of Political Stability: England, 1675–1725* (Boston, 1967), xvii.

the end of the 1960s the societies of Western Europe seemed more cohesive, humdrum, and routine than either those who feared change or those who longed for it would have predicted.

The key to this stability lies in both postwar eras, the period after World War I as well as that after World War II. Although the years after the first war did not bring enduring stabilization, neither did they produce the radical economic and social change that Left and Right had expected. Outside Russia the first war opened the way only to limited upheaval, conservative reconstruction, or, in some cases, counterrevolution. With the end of the second war, as Taylor's prognosis suggested, many observers again anticipated a major social transformation. This time the postwar years brought not only an ebbing of radicalism but at least a generation of political and economic stability as well. Yet that mid-century stability rested upon the cumulative achievements of both postwar eras. Together the postwar intervals comprised two chapters in a single half-century effort by reform-minded and conservative elites to exploit postwar circumstances for a successful restructuring of the hierarchies they dominated.

GIVEN THE OBJECTIVE OF HISTORICAL COMPARISON, the two periods are usefully envisaged as complementary and parallel alike. Complementary (as is stressed below) in that each made its own distinct but partial contribution to the process of channeling change. Parallel in that key political and economic developments tended to recapitulate themselves. The recurring elements after both wars demonstrate that, although many problems were different, the same underlying political cleavages, enduring class and industrial conflicts, and continuing economic dilemmas remained. As in earlier postwar transitions, each period witnessed a swing from radical challenge to political consolidation. Such a trajectory had marked Europe in the aftermath of the wars of the French Revolution and Napoleon, Russia following the Crimean War, Italy, Prussia, and Austria after the wars of unification, the United States after its Civil War, and France again in the wake of 1870, Spain after 1898, and Russia after 1905. The periods after the two world wars likewise reveal certain parallels.

Consider, first, the comparable political developments. Just as radical or reformist forces of the Left seemed ready to impose extensive changes and then lost their impetus between 1918 and 1921, so the Resistance-born coalitions of Communists, Socialists, Catholics, and liberal democrats initiated reforms but collapsed by 1947–48. In both cases this disarray followed early polarization within the working-class parties and unions. From the viewpoint of the moderates, Soviet-oriented leaders grew ruthlessly opportunistic and sectarian; reverse the perspective and Social Democrats appear preoccupied with Bolshevism or communism. After both wars, too, the respective Catholic parties—the German Zentrum and the Italian Popolari after 1918, the diverse Christian Democrats after 1945—also retreated from their earlier commitments to boldly proclaimed economic reforms. Catholic trade-union leaders and left intellectuals lost out to spokesmen for middle-class stability, the Church hierarchy, or “social-market liberalism.”

A careful distinction is necessary here. After 1945, plans to supersede capitalism yielded to efforts to reinvigorate economic liberalism. Yet liberal party organizations continued the long-term decline that had originated even before World War I. This attrition hurt both right- and left-wing variants of liberalism, although the Right could fall back upon the economic interest groups it dominated and the Left still controlled influential journalistic outposts. Electoral support, however, was a different story. Voting results were prevailingly disappointing. In 1946 Italian laissez-faire Liberals and the reformist Actionists together polled no more than 8 percent of the electorate. The French non-Marxist, non-Catholic Left had brilliant writers but few voters. Belgian Liberal deputies were returned at roughly half of their prewar strength with about 9 percent of the popular poll, and the revived Liberal Democratic party in West Germany (today's Free Democrats), with its 9.5 percent in Landtag elections and 12 percent of the first Bundestag, remained comparable to voting results of the combined Democratic (Staatspartei) and People's parties in the late Weimar Republic.³

Just as striking as the draining of energy on the Left in the respective postwar years was the recapitulation of key industrial and monetary developments. Certainly the economy of the era after 1948 became far more robust than the ephemeral prosperity of the late 1920s. Nonetheless, some of the same dilemmas and solutions marked both recoveries. By the mid-1920s Americans were finally helping ease Europe's postwar balance-of-payments difficulties by the enthusiastic purchase of European bonds. At the same time, leading bankers on both sides of the Atlantic pressed for currency stabilization and monetary convertibility on the basis of the gold-exchange standard: the Reichsmark was anchored in late 1924, sterling in April 1925, the lira in 1927, and the French franc (legally re-established exclusively on a gold base) in 1928. The laboriously negotiated tariff compromises and trade treaties of the latter 1920s along with such interindustry agreements as the Entente Internationale de l'Acier advanced the integration of the major Continental steel and chemical producers. Agreements between industries across frontiers encouraged mergers and concentration within the component national economies. In a similar sequence after World War II, the European Recovery Program of 1948-51 and subsequent Mutual Security assistance provided American credits to compensate for Europe's massive dollar deficit. The European Payments Union, the product of negotiations extending from 1948 to 1951, worked toward renewed currency convertibility. The Coal-Steel Community of the early 1950s reinforced the capitalist revival of the second postwar period.⁴

³ For a useful tabulation of voting results, see Derek W. Urwin, ed., *Elections in Western Nations, 1945-1968*, University of Strathclyde, Survey Research Center, *Occasional Papers*, nos. 4-5 (Glasgow, n.d.).

⁴ For the negotiations of the 1920s, see Charles S. Maier, *Recasting Bourgeois Europe: Stabilization in France, Germany, and Italy in the Decade after World War I* (Princeton, 1975), 516-45; and Jacques Bariéty, "Das Zustandekommen der Internationalen Rohstahlgemeinschaft (1926) als Alternative zum misslungenen 'Schwerindustriellen Projekt' des Versailler Vertrages," in Hans Mommsen et al., eds., *Industrielles System und politische Entwicklung in der Weimarer Republik* (Düsseldorf, 1974), 552-68. For the negotiations between coal and steel producers after World War II, the material in the steel trusteeship papers at the Koblenz Bundesarchiv [hereafter, BA], B 109/97, is revealing; these papers are complemented by the memoranda of meetings included in

Obviously, there were crucial differences between the two postwar eras; to discern parallels is not to claim identities. After the First World War, to cite just a first salient difference, the political Right emerged more militant than before 1914. Fascism drew upon a striking force of veterans inured to violence and contemptuous of civilian virtues. After the Second World War, fascism was discredited and even traditional conservative nationalism rejected. The psychological impact of the fighting did not create nuclei of Arditi, Free Corps recruits, or others addicted to paramilitary violence. For most soldiers the second war impelled instead a search for private fulfillment: "the happy obscurity of a humdrum job and a little wife and a household of kids," according to Bill Mauldin, or, a front away, the return to "the mountains of the Caucasus, the exciting blue smoke of the foothills . . . , the sweet faces of loved ones."⁵ What analogue existed after 1945 to the *trinceismo*, the glorification of the trenches of World War I, was the partisans' mountain ordeal: a trial that Resistance spokesmen claimed was moral justification for a new elite, although without any encouragement for a continuing cult of violence. Indeed, the distinction in 1914–18 between front soldiers mired down in brutalizing combat and male civilians at home who sometimes enjoyed cushy, protective berths—the so-called *embusqués* or *imboscanti*—dissolved in 1939–45 with the rapid movement of troops, the air attacks on civilian targets, and the hardships of occupation. Almost 50 percent of Europe's dead in the second war were civilians, compared to about 5 percent in the first.⁶ These factors all contributed to limiting the potential of any veterans-based right radicalism. Except for the recurring but small German nationalist splinters, achieving at best 8 to 10 percent electoral support at the Land level (and about 2 percent in national polls), the search for right-wing movements after 1945 yields only ambivalent possibilities: the Gaullism of 1947 and the Uomo Qualunque of southern Italy, a sort of pre-Poujadism that rejected the moralistic claims of the Resistance Left. This failure of the neofascist Right to emerge in greater strength was a major surprise of postwar European politics.⁷

the archives of the Compagnie de Pont-à-Mousson at La Châtre [hereafter, PAM], boxes 70669, 70671, 70690–91, 77042. Also see William Diebold, Jr., *The Schuman Plan: A Study in Economic Cooperation, 1950–1959* (New York, 1959). For monetary negotiations, see Stephen V. O. Clarke, *Central Bank Cooperation, 1924–1931* (New York, 1967); Sir Henry Clay, *Lord Norman* (London, 1957); L. V. Chandler, *Benjamin Strong, Central Banker* (Washington, 1958); W. A. Brown, Jr., *England and the New Gold Standard, 1919–1926* (New Haven, 1929), and *The International Gold Standard Reinterpreted, 1914–1934*, 2 vols. (New York, 1940); Donald E. Moggridge, *British Monetary Policy, 1924–1931: The Norman Conquest of \$4.86* (Cambridge, 1972); and Gerd Hardach, *Weltmarkt-orientierung und relative Stagnation: Währungspolitik in Deutschland, 1924–1931*, *Schriften zur Wirtschafts- und Sozialgeschichte*, vol. 27 (Berlin, 1976). For post-1945 negotiations, see William Diebold, Jr., *Trade and Payments in Western Europe* (New York, 1952); J. Kummell, *De Ontwikkeling van het Internationale Betalingsverkeer* (Leiden, 1950); Robert Triffin, *Europe and the Money Muddle* (New Haven, 1957); and Raymond F. Mikesell, *Foreign Exchange in the Postwar World* (New York, 1954).

⁵ As quoted in John Morton Blum, *V Was for Victory: Politics and American Culture during World War II* (New York, 1976), 70, 73. For the attitudes and political organization of veterans, only a minority of whom became radically antidemocratic, see James M. Diehl, *Paramilitary Politics in Weimar Germany* (Bloomington, Ind., 1977); Volker R. Berghahn, *Der Stahlhelm, Bund der Frontsoldaten, 1918–1935* (Düsseldorf, 1966); Robert G. L. Waite, *Vanguard of Nazism: The Free Corps Movement in Postwar Germany, 1918–1923* (Cambridge, Mass., 1952); Giorgio Sabatucci, *I combattenti nel primodopoguerra* (Bari, 1974); and Antoine Prost, *Les Anciens combattants et la société française, 1914–1939*, 3 vols. (Paris, 1977), esp. volume 3: *Idéologies et mentalités*.

⁶ Gordon Wright, *The Ordeal of Total War, 1939–1945* (New York, 1968), 264.

⁷ For the Gaullism of 1947, see Jean Touchard, *Le Gaullisme, 1940–1969* (Paris, 1978), 98–133. On Uomo Qualunque, see Sandro Setta, *L'Uomo Qualunque, 1944/48* (Bari, 1975). And, for post-1945 Germany, see Kurt P. Tauber, *Beyond Eagle and Swastika: German Nationalism since 1945*, 2 vols. (Middletown, Conn., 1967).

Only in retrospect is it discernible that even under the collaborationist regimes conservative elements had to rethink the economic role of the state and the future relationship of capital and labor. To cite just the French situation (although analogues existed in the Netherlands and the Salò Republic), awareness that the Vichy regime was doomed and mass upheaval likely prompted the industrialists summoned by the Conseil Supérieur de l'Économie Industrielle et Commerciale to search for a "factory community" that would provide a "balanced solution" between "yesterday's capitalism" and "collectivism." Such explorations, however, could build upon more than fear of postwar revolution. They carried forward some of the heterodox notions of economic planning that dissenting socialists and conservative intellectuals alike had outlined in the 1930s.⁸

A major condition for a more flexible Right was the fact that the Left too debarked differently after the second war. Between 1918 and 1921 the European working classes had first surged into spontaneous demonstrations, had then waged long, disciplined mass strikes, and had finally retrenched in frustration and divided. Much of their insurrection followed from the intensified labor discipline the war imposed as well as progressive ideological alienation from its national objectives. The second war imposed some of the same ordeals within the factory, but the German occupation made the factory a less central source of oppression. The heirs of the working-class leadership that had come to oppose the first war by 1917 urged active resistance to the Germans after June 1941, so that the second war was less an alien upper-class cause than an arduous wait for liberation. Its conclusion thus brought a different tempo of working-class cooperation and protest. Western Communists played down any radical economic transformation that outran the broad Resistance consensus on purges and the nationalization of key industries or those tainted by their owners' collaboration. Instead Communist leaders stressed anti-Nazi unity (until the final defeat of Germany) and continuing production, even at the cost of harsh industrial discipline. "The bonus per ton is evil," wrote one CGT leader in March 1945 about detested pay differentials, "but coal is necessary." Maurice Thorez insisted to coal miners at Waziers in July 1945 that production itself was a demonstration of solidarity, militancy, and working-class power.⁹ "Only by working, only by working hard will we be able to overcome this situation [of hardship]," the secretary of the Milan Chamber of Labor told the factory council of Magneti Mirelli in early 1946. "We all seek socialism. But do you believe that we can

⁸ Conseil Supérieur de l'Économie Industrielle et Commerciale, Commission Nr. 4, Procès-Verbal de la séance du 5 novembre 1943, PAM, box 70411; and Report to the Minister, July 17, 1944, *ibid.* Also see Richard Kuisel, "Vichy et les origines de la planification économique (1940-1946)," *Le Mouvement Social*, 98 (1977): 77-101; Jacques Amoyal, "Les Origines socialistes et syndicalistes de la planification en France," *ibid.*, 87 (1974): 137-69; and, on the labor issue, Jacques Julliard, "La Charte du Travail," in Jeanine Bourdin, ed., *Le Gouvernement de Vichy, 1940-1942: Colloque de la Fondation Nationale des Sciences Politiques* (Paris, 1972), 157-210.

⁹ L. Delfosse, in *La Tribune des Mineurs*, March 18, 1945, as quoted in Jean Bouvier, "Région et Nation: Inflation, reformes de structures, nationalisation des houillères, et crise sociale," *Actes du Colloque de l'Université de Lille III, 2-3 novembre 1974: La Libération du Nord et du Pas-de-Calais, 1944-1947* [hereafter, *Colloque de Lille*], in *Revue du Nord*, 57 (1975): 609. For Communist policies, see J.-P. Hirsch, "La Seule voie possible: Remarques sur les communistes du Nord et du Pas-de-Calais de la Libération aux grèves de novembre 1947," *ibid.*, 563-78, which contains an extensive discussion of Thorez's celebrated Waziers appeal, July 21, 1945.

socialize poverty?" Communists, warned Jacques Duclos, as he condemned the 1946 Socialist-supported strikes of French civil servants, had to demonstrate "that democracy is a regime of order, a regime of tranquility and of work."¹⁰

The open question in France, Belgium, and Italy (to the extent that the Anglo-American occupation would have permitted) was whether an angry and long-repressed working class would explode in a spontaneous radicalism with plant seizures, local "socialization," and summary trials. Communist pressure for carrying through purge procedures probably helped contain grass-roots grievances. In fact, whether in France, Italy, Belgium, Holland, or Bavaria (under American auspices), the purges became more and more restricted. Categories of guilt seemed to blur hopelessly, and moderates came to grasp that trying business leaders for cooperation with the Germans could have radical consequences, or, in the words of one *Esprit* intellectual who advocated it, "The purge of the economic sector entails overturning all property relations."¹¹ Purges were thus wound down short of any major upheaval, and the emphasis upon sifting individuals probably diverted effort from institutional transformation—although originally the Left had envisaged *épuration* as a mode of collective change.

Perhaps, however, the major force for preventing ideological polarization after World War II was neither the chastened Right nor the tempered Left but the new Christian Democratic parties of the center. For the crucial three years after 1944, left Catholicism with its declared hostility to liberal capitalism seemed ascendant. The appearance was deceptive in the long run, but it served well to contain otherwise radical currents in the flux of the immediate postwar period. Konrad Adenauer could swallow and survive the radical-sounding Ahlen Program of the Westphalian Christian Democrats in 1947, understanding that it kept the CDU from appearing reactionary; Alcide De Gasperi ultimately profited from the mass base organized by Catholic labor leader Achille Grandi; and the French MRP accepted nationalization but, except for collaborators, insisted upon compensation.¹² In Italy and Belgium the prolonged controversy

¹⁰ Milanese secretary of the Chamber of Labor, as quoted in Febo Guizzi, "La Fabbrica italiana Magneti Marelli," in Luigi Ganapini et al., *La ricostruzione nella grande industria: Strategia padronale e organismi di fabbrica nel Triangolo, 1945-1948* (Bari, 1978), 280; and Duclos, as quoted in Alain Bergonieux, *Force Ouvrière* (Paris, 1975), 55.

¹¹ G. Zérappa, "Le Problème politique français," *Esprit*, December 1944, as quoted in Michel Winock, *Histoire politique de la revue "Esprit"* (Paris, 1975), 260. On the purges, see Peter Novick, *The Resistance versus Vichy* (New York, 1968); Robert Aron, *Histoire de l'épuration*, 3 vols. (Paris, 1967-75); D. Laurent et al., "Sur l'épuration dans le Nord et le Pas-de-Calais," *Colloque de Lille*, in *Revue du Nord*, 57 (1975): 365-80, 623-36; Lutz Niethammer, *Entnazifizierung in Bayern: Säuberung und Rehabilitierung unter amerikanischer Besatzung* (Frankfurt a/M, 1972); and Marcello Flores, "L'Epurazione," in Istituto Nazionale per la Storia del Movimento di Liberazione in Italia, *L'Italia dalla liberazione alla repubblica; del convegno internazionale . . . 26-28 marzo 1976* (Milan, n.d.), 413-67. Also see Guizzi, "La Fabbrica italiana Magneti Marelli," 245-72; and Valerio Castronovo, *Giovanni Agnelli* (Turin, 1971), 671-88. Also see the reports from U.S. diplomats on the slowing of the Belgian and Dutch purges, National Archives, Washington, Record Group 59 [hereafter, NA-RG 59], including the report by Charles Sawyer, May 29, 1945, NA-RG 59,855.00/5-2945; by Theodore Achilles, June 11, 1946, *ibid.*, 855.00/6-1146; and by J. Webb Benton from the Hague, August 15, 1946, *ibid.*, 856.00/8-1546.

¹² For Adenauer's views, see Sozialausschuss der CDU, February 21-22, 1947, in Friedrich-Ebert-Stiftung, Bonn, Hensler Nachlass, 16. Also see Gerold Ambrosius, *Die Durchsetzung der sozialen Marktwirtschaft in Westdeutschland, 1945-1949* (Stuttgart, 1977); and Konrad-Adenauer-Stiftung, *Konrad Adenauer und die CDU der britischen Besatzungszone, 1946-1949* (Bonn, 1975), 46-47, 288-89. On Grandi, see Benedetto de Cesaris, "Cattolici,

over the fate of the discredited monarchs helped the Catholic parties accommodate both Left and Right. As the American embassy reported from Brussels, the Christian Social party, by defending the rights of Leopold III, could retain the allegiance of Belgian conservatives, even while letting its trade unionists champion social reform, and could thus provide "all things to all men who believe in the Roman Catholic religion."¹³ This capacity naturally undermined the radical élan of Christian democracy but did allow the movement to serve as an integrating force for moderation.

Domestic party developments obviously took place under the shadow of the great powers. The overwhelming difference between 1918 and 1945 was the continuing intervention of the United States and the Soviet Union in their respective spheres of influence. But in Western Europe, American aid, with its attendant pressure, was only one of many factors abetting liberal reconstitution. The discrediting of the European Right, the fear of Communist motives and the Soviet Union that replaced Popular Front effusions, and the desire on the part of both Christian Democrats and Social Democrats to establish moderate welfare states were powerful impulses on their own. They alone sufficed to make 1945 different from 1918.

DIFFERENT, BUT NOT SEPARATE. Both postwar periods, as noted, formed part of a continuing effort at stabilization, a search that was sufficiently active and persistent (and rewarded finally with sufficient success) to comprise a major theme of twentieth-century Western European history. Stabilization, however, for whom? And of what? Stabilization meant not so much preserving liberal procedures as re-establishing the overlapping hierarchies of power, wealth, and status that can be loosely termed "capitalist." In an age of mass suffrage, these challenged hierarchies had to be defended less in terms of custom than results—that is, their performance for society as a whole. Increasingly, performance included the maintenance of economic welfare. The Depression led voters to shatter the Western political coalitions of the 1920s even when it did not destroy democratic regimes. Distress forced governments in the 1930s to become employers of last resort; by the 1950s they were called upon to assure continuing economic growth as well as high employment at a given level of national income. Stabilization thus entailed a dual task. It meant re-establishing the contested legitimacy of European social and economic elites—buttressing the hierarchies that even in an age of mass voting still presupposed that only small minorities could share the prerogative of directing human labor. Justifying inequality, in turn, required satisfying criteria of economic performance: figuratively and literally delivering the goods. Although they had to broaden their recruitment and recognize new spokesmen for organized labor, by and large the elites superin-

eredità 'popolari,' e nuovo stato," in *Problemi del movimento sindacale in Italia, 1943-1973: Annali della Fondazione Giangiacomo Feltrinelli*, 16 (1976): 229-39. For the MRP stance, see the discussions of Bidault and Menthon with Communist and Socialist leaders, January 23, 1945, *Colloque de Lille*, in *Revue du Nord*, 57 (1975): 596-97.

¹³ Jefferson Patterson to the Department of State, August 20, 1945, NA-RG 59, 855.00/8-2045.

tending Western society met these related conditions for stability—those of legitimation and those of production. But they did not meet both conditions at once.

Instead, Europe's elites resolved their difficulties *seriatim*, such that each effort of postwar stabilization overcame one of the two challenges. With the 1920s came not a total, but a nevertheless impressive, response to the ideological attack upon the legitimacy of capitalist hierarchies as hierarchies. That is, the leaders of the 1920s rallied with persuasive justifications of capitalist entrepreneurship. They ended up rejustifying not so much ownership *per se* as a hierarchy of managerial power that preserved the essentials of control. Nonetheless, the 1920s did not solve the economic dilemma of ensuring continuous production and high employment. That task was left to the second postwar period. Only by the 1950s were the afflictions that undermined capitalist stability effectively overcome as a whole. The cumulative achievement required the institutional flux that was left in the wake of not one but two wartime upheavals.

In what sense can it be maintained, however, that developments of the 1920s served durably to reinforce the legitimacy of European capitalism? In light of mass unemployment, the taint of wartime collaboration, and the wave of socialist aspirations incorporated in the Resistance, did not capitalism seem as shaken, vulnerable, and problematic after World War II as ever before—hence A. J. P. Taylor's verdict? In fact, however, the Left's programs after World War II did not often go so far as the challenges of 1918–21.¹⁴ This does not mean that the Left was universally stronger earlier. Although in Germany and Italy social revolutionary outcomes had been more feasible after the First than the Second World War (if only because no occupying forces were present), had it chosen to exploit its power, the French Left possessed a more commanding position in late 1944 than it had controlled in 1918. In Britain the protests of 1918–19 that looked toward a syndicalist socialism were succeeded in 1945 by the more solid, if more moderate, triumph of the Labour party. In short, the relative strength of the Left in the respective postwar periods depended to a great extent upon the particular national situation. The programs of the Left, however, often remained a less clearcut challenge after 1944 than they were after 1918. They aspired less to overturn bureaucratic and economic control than to attain public ownership of key industries. By 1945, however, ownership was a less crucial issue than earlier for many sectors that the Left targeted for nationalization. The earlier socialist challenge that followed in the wake of the Bolshevik Revolution with its innovation of soviets was probably more fundamental. What the participants in the massive strikes and insurrections of 1918–21, the militants at party and union congresses, and the remarkable socialist theorists of the early 1920s urged in aggregate was not merely the centralization of important industries in

¹⁴ On this point, see some of the recent surveys of this period, including Francis Carsten, *Revolution in Central Europe, 1918–1919* (London, 1972); Charles L. Bertrand, ed., *Revolutionary Situations in Europe, 1917–1922: Germany, Italy, Austria-Hungary*, Proceedings of the Second International Colloquium of the Interuniversity Center for European Studies (Montreal, 1977); and *Rivoluzione e reazione in Europa, 1917/1924: Convegno storico internazionale*, Perugia, 1978 (Rome, 1978).

the hands of the state; this demand came from moderate Social Democrats. Instead, they criticized managerial control of the workplace and of production regardless of ownership; and, by extension, they challenged the chains of command of the Western economies from top to bottom.

These movements failed in the West for many reasons. They were rooted in the shop steward organizations of the Clydeside, the factory grievance committees established during the war to smooth labor relations, which in turn helped generate the *consigli di fabbrica* of Turin and the *Räte* in Germany and Austria. Some spokesmen for these councils envisioned a syndical reorganization of the economy and politics. But often their militancy derived from the more conservative impulses of defending the work skills and artisanal independence still conserved under factory roofs against degrading standardization of tasks and wartime "dilution" (the hiring of unskilled replacements, sometimes women).¹⁵ Moreover, the councils comprised a strong movement only in a few industrial regions, and their revolts exploded out of phase with each other. The movement, moreover, appears to have evoked the least resonance in France, which would still have had to be the keystone of any general West European transformation. In France, reformist socialists as well as industrial leaders restricted the mandate of factory delegates, while after the armistice radicals spilled into street demonstrations that were militant but diffuse and finally settled on a program for nationalization of the railroads.¹⁶ Likewise in Britain, Labour militants came to focus upon takeover of the coal industry. In Germany, the councils emerged during revolution but often just to take charge of factories, regiments, or towns in which central authority crumbled. When German council champions took up explicitly socialist goals, they incurred drastic repression, as in Munich in April 1919 or in the Ruhr after the Kapp Putsch.¹⁷

Trade-union leaders, moreover, remained cool toward alternative modes of representation, fearing that the new councils would undercut their long, patient

¹⁵ On the resistance of skilled workers, see James Hilton, *The First Shop Stewards' Movement* (London, 1973); Bertrand Abhervé, "Les Origines de la grève des métallurgistes parisiens, juin 1919," *Le Mouvement Social*, 93 (1975): 75–85; and David Montgomery, "The 'New Unionism' and the Transformation of Workers' Consciousness in America, 1909–1922," *Journal of Social History*, 7 (1974): 509–29. Also see Carmen J. Sirianni, "Workers' Control in the Era of World War I: A Comparative Analysis of the European Experience," *Theory and Society*, 9 (1980): 29–88; and Martin Clark, *Antonio Gramsci and the Revolution that Failed* (New Haven, 1977).

¹⁶ Abhervé, "Les Origines de la grève des métallurgistes parisiens"; Nicholas Pappayanis, "Masses revolutionnaires et directions reformistes: Les Tensions au coeur des grèves des métallurgistes français en 1919," *Le Mouvement Social*, 93 (1975): 51–73; and Gilbert Hatry, "Les Délégués d'atelier aux Usines Renault," in Patrick Fridenson, ed., *1914–1918, l'autre front: Cahiers du "Mouvement Social"*, 2 (Paris, 1977): 221–35. Also see the older surveys, Roger Picard, *Le Mouvement syndical durant la guerre* (Paris, 1927); and William Oualid and Charles Picquenard, *Salaires et tarifs, conventions collectives, et grèves: La Politique du Ministère de l'Armement* (Paris, 1928).

¹⁷ On Britain, in addition to Hinton's *The First Shop Stewards' Movement*, see Branko Pribicevic, *The Shop Stewards' Movement and Workers' Control* (Oxford, 1959); Arthur Marwick, *The Deluge* (New York, 1970), 56–76, 203–09; and G. D. H. Cole, *Labour in the Coal-Mining Industry, 1914–21* (Oxford, 1923). On Germany, see Eberhard Kolb, *Die Arbeiterräte in der deutschen Innenpolitik, 1918–1919* (Düsseldorf, 1962), and "Rätewirklichkeit und Räte-ideologie in der deutschen Revolution von 1918–1919," in Kolb, ed., *Vom Kaiserreich zur Weimarer Republik* (Cologne, 1972), 165–84; Reinhard Rürup, ed., *Arbeiter- und Soldatenräte im rheinisch-westfälischen Industriegebiet* (Wuppertal, 1975); Peter von Oertzen, *Betriebsräte in der Novemberrevolution* (Düsseldorf, 1963); Erhard Lucas, *Märzrevolution im Ruhrgebiet*, vol. 1 (Frankfurt a/M, 1970), and *Märzrevolution 1920*, vol. 2 (Frankfurt a/M, 1973); and Georg Eliasberg, *Der Ruhrkrieg 1920* (Bonn, 1974).

struggle to speak for labor. Bourgeois politicians such as David Lloyd George in 1919 or Giovanni Giolitti in 1920 deflected protests into cumbersome committees, which finally generated compromise proposals for co-determination that commanded no adherence and were soon shelved (much like the recent Bullock Commission in Britain). Supple industrial leaders, such as Milanese banker and electrical magnate Ettore Conti or Rhenish lignite industrialist Paul Silverberg, similarly exploited such spurious concessions.¹⁸

Still, given the limitations of the movement, the council episodes suggested that bourgeois concepts of rational economic and political authority were all terribly vulnerable. The dramas staged at Fiat or Renault or the mines of Essen were frightening not primarily because they may have attained an ephemeral success but because they suggested that only force, not consensus, stood in the way of a collectivist alternative. At stake, therefore, was bourgeois legitimacy as well as naked control. Bourgeois response, thus, had to go beyond mere repression. Counterstrategies had to operate on plant and national planes, micro- and macro-levels simultaneously. The need to reassert authority within the factory gave renewed impetus to plans for scientific management, which would further centralize factory authority by differentiating tasks "down to the tiniest detail," as some French sponsors defined their Taylorite efforts.¹⁹

Acceptance of this technocratic functionalism required conservative flexibility, and business as well as political milieus divided between progressives and reactionaries. The reactionaries distrusted industry-wide organization and insisted on the prerogatives of ownership, asserting what the Germans called their *Herr-im-Hause* domination. But the more fruitful approach was to build upon the potential for cooptation that wartime labor-management agreements and the unavowed brotherhood of wage-price spirals had encouraged after 1914.²⁰ As might be expected, the industrial "progressives" were less fixated on ownership, more concerned with managerial expertise; they were multi-divisional foxes rather than single-factory lions. Building upon his wartime organizational efforts, Walther Rathenau forcefully defended entrepreneurial leadership, while outlining complex schemes for capitalist self-government and planning. Later in the decade, Alfred Mond, organizer of the Imperial Chemical cartel and Ernest Mercier, an architect of French electrical networks, pursued related visions (as did Herbert Hoover in the United States).²¹ Other spokesmen throughout the

¹⁸ See Charles S. Maier, *Recasting Bourgeois Europe* (Princeton, 1975), chap. 3.

¹⁹ "Concours pour l'application du Système Taylor dans les Mines et Usines de la Société de Pont-à-Mousson: Préambule," PAM, box 18936.

²⁰ Gerald Feldman, "German Business between War and Revolution: The Origins of the Stinnes-Legien Agreement," in Gerhard A. Ritter, ed., *Entstehung und Wandel der modernen Gesellschaft: Festschrift für Hans Rosenberg zum 65. Geburtstag* (Berlin, 1978), 312-41, and *Iron and Steel in the German Inflation, 1916-1923* (Princeton, 1977), 91; Charles A. Gulick, *Austria from Habsburg to Hitler*, 2 vols. (Berkeley and Los Angeles, 1948), 1: 150-57; and Charles S. Maier, "The Politics of Inflation in the Twentieth Century," in Fred Hirsch and John Goldthorpe, eds., *The Political Economy of Inflation* (Cambridge, Mass., 1978), 49-52.

²¹ Walther Rathenau, *Von kommenden Dingen* (1916), and *Die neue Wirtschaft* (1917), volumes 2 and 3 of his *Gesammelte Schriften* (Berlin, 1918); Alfred Moritz Mond, *Industry and Politics* (London, 1927); Hector Bolitho, *Alfred Mond, First Lord Melchett* (London, 1933), 313-18; and Richard Kuisel, *Ernest Mercier, French Technocrat* (Berkeley and Los Angeles, 1967). Also see Martin Fine, "L'Association Française pour le Progrès Social (1927-1929)," *Le Mouvement Social*, 94 (1976): 3-29.

1920s derived from the important interindustry associations—less businessmen than their organizers and lobbyists: François Poncet of the Comité des Forges with his sleek defense of technocratic inequality; Gino Olivetti of Confindustria, who from even before the war was to emphasize that only the industrialist could “technically order the factory according to a pre-established plan”; his successor Antonio Benni, who insisted that industry was “not personified by the capitalist or the stockholder but by its directors, by its chiefs, and by the organizers of the enterprise.”²² Industry, moreover, became the paradigm for political society in general, as, for instance, when Ernest Mercier sought to rally managerial expertise in the above-party Redressment Français or Alfred Mond organized the Mond-Turner talks with trade-union leaders in the wake of the British General Strike.

These initiatives and self-justifying notions were hardly widespread enough to reorder industrial organization, any more than the council movement had revolutionized the workplace. Nonetheless, celebrators and critics alike felt that scientific management represented a decisive economic and social breakthrough,²³ and the economic circumstances of the late 1920s powerfully reinforced this new legitimization of capitalism. The stabilization of currencies on the gold-exchange standard, renewal of intense international competition, and concern about saturation of home markets all made “rationalization” more urgent. Rationalization was a concept that comprised market-sharing agreements across frontiers and within domestic economies plus parallel efforts to lower the burden of wages and other costs through investment, technical improvements, and mergers. At the same time industrial leaders sought legitimization for their power, whether it derived from the right to lay off workers in a cyclical downturn or from their collaboration with an authoritarian regime as in Italy. The managerial mystique evoked widespread enthusiasm, assumed a truly cultic importance precisely because it was a modern and supposedly class-neutral alternative to the immediately preceding socialist attack on industrial hierarchies.

“This Taylorization is connected with the problem of lowering overall costs,” noted Marcel Paul, a Pont-à-Mousson manager, when his firm embarked upon the venture in the late 1920s.²⁴ Scientific management supposedly promised a painless method of cost cutting, although it often just meant speed-ups or extra

²² André François Poncet, *Reflexions d'un républicain moderne* (Paris, 1925); Olivetti, as quoted in Franklin Adler, “Factory Councils, Gramsci, and the Industrialists,” *Telos*, 31 (1977): 79; and Benni, as quoted in Maier, *Recasting Bourgeois Europe*, 567. Also see Franklin Adler, “Italian Industrialists and Radical Fascism,” *Telos*, 30 (1976–77): 193–201.

²³ For André Philips’s analysis of the central role of scientific management in American economic achievement, see his *Le Problème ouvrier aux États-Unis* (Paris, 1927). For the enthusiasm evoked by what I call the “managerial mystique,” see Edmond Giscard d’Estaing, “Le Néocapitalisme,” *Revue des Deux Mondes*, August 1, 1928; Paul Devinat, *Scientific Management in Europe*, ILO Studies and Reports, ser. B, no. 17 (Geneva, 1927); and *La Prospérité: Revue trimestrielle de l’organisation scientifique* (1928–), an ebullient magazine that was briefly published by Michelin.

²⁴ Marcel Paul to Jean Cavalier, May 19, 1928, PAM, box 41595. On the thrust of rationalization, see Robert Brady, *The Rationalization Movement in German Industry* (Berkeley and Los Angeles, 1933); Giulio Sapelli, “L’Organizzazione ‘scientifica’ del lavoro e innovazione tecnologica durante il fascismo,” *Italia Contemporanea*, 28 (1976): 3–28; and Paola Fiorentini, “Ristrutturazione capitalistica e sfruttamento operaio in Italia negli anni ’20,” *Rivista Storica del Socialismo*, 10 (1967): 134–54.

hours. The unions, however, had already exhausted their capacity for resistance during fruitless labor struggles, in France during 1920, in Italy from 1920 through 1922 and less overtly in 1924–26, in Germany after the inflation and again by 1928, and in Britain during 1921 and 1926. By the late 1920s, moreover, a generation of moderate labor spokesmen emerged who honestly believed in collaboration: trade unionists in Germany and the United States, the aging Albert Thomas at the Geneva International Labor Office, the younger Walter Citrine, and Ernest Bevin, who saw his job of “the large scale organization of labor” as akin to that of the industrial manager.²⁵

The Left never again challenged the premise that production was a question for managers and engineers with the same vigor that they had immediately after the first war. Even when the close of World War II seemed to offer renewed opportunity, plans to reorganize the factory and control production remained relatively undeveloped. In France, workers revolted against the coerciveness of the Occupation and sought to oust patrons they identified as both collaborators and exploiters. But their efforts yielded only limited success, and the bitterness of the later strikes in 1947 and 1948 testified to the frustration of aspirations raised at the Liberation. The Communists did support new schemes for a workers’ voice in the tripartite management boards (representing management, labor, and the state) for the nationalized industries, urged by Minister of Industry Marcel Paul. But they had to retreat in the face of MRP and Socialist counter-measures to ensure a more technical supervision; nor was it clear that the PC had really wanted more than its own industrial barony.²⁶ Italian workers were perhaps most consistent in reviving factory representation through the *consigli di gestione*. Communist spokesmen, however, came to define these councils as a structure for giving the workers a stake in production. They were not intended to replicate Gramsci’s revolutionary factory councils.²⁷ And, in Germany, while co-determination as sought in the mining and metal industries may well have represented a creative and innovative demand, it still remained an effort more to share in the control of traditional managerial functions rather than to overthrow them. The left-wing SPD spokesman Viktor Agartz developed the most

²⁵ “The opposition of leaders of labor to bonafide scientific management has practically disappeared, and during recent years there has been noteworthy cooperation between scientific management leaders and labor leaders”; H. S. Person, “Scientific Management,” Industrial Relations Committee Report, February 15, 1928, AFL Papers, Florence Thorne Collection, 117/8A, box 18, State Historical Society of Wisconsin, Madison, Wisc. Also see Milton J. Nadworny, *Scientific Management and the Unions, 1900–1932* (Cambridge, Mass., 1955); Philips, *Le Problème ouvrier aux États-Unis*, 556; Martin Fine, “Albert Thomas: A Reformer’s Vision of Modernization, 1914–1932,” *Journal of Contemporary History*, 12 (1977): 545–64; Madeline Rebérioux and Patrick Fridenson, “Albert Thomas, pivot du reformisme français,” *Le Mouvement Social*, 87 (1974): 85–97; and Alan Bullock, *The Life and Times of Ernest Bevin*, volume 1: *Trade Union Leader, 1881–1940* (London, 1960), 396.

²⁶ See Etienne Dejonghe, “Les houillères à l’épreuve, 1944–1947,” *Colloque de Lille*, in *Revue du Nord*, 57 (1975): 643–66. On nationalization schemes, see Mario Einaudi et al., *Nationalization in France and Italy* (Ithaca, N.Y., 1955), 96–105.

²⁷ For Emilio Sereni’s exhortation, see Guizzi, “La Fabbrica italiana Magneti Marelli,” 252; and, on the role of post-1945 councils, see Paride Rugafori, “La ‘Ricostruzione’ in una grande azienda IRI in crisi: L’Ansaldo (1945–1948),” in Ganapini et al., *La Ricostruzione nella grande industria*, 428–444; and Giulio Sapelli, “Industriali e lotta di classe a Torino (1945–1947),” *ibid.*, 445–527. Also see Liliana Lanzardo, *Classe operaia e partito comunista alla Fiat: La Strategia della collaborazione* (Turin, 1971). For a good survey, see F. Levi et al., *Il Triangolo industriale tra ricostruzione e lotta di classe, 1945–1948* (Milan, 1974).

extensive concepts of “economic democracy” but quickly declined in influence in his own party once the Federal Republic was constituted.²⁸ In short, the second postwar era did not resume the fundamental ideological challenge to managerial control of twenty-five years earlier. The first postwar restoration had largely confirmed the premise that the modern industrial order must operate under hierarchical chains of command, like an army or bureaucracy. The presumption of technical rationality legitimized the economic power that ownership alone could not.

SUBDUING LABOR’S BID to control the organization of production and, by extension, to make economic authority democratic was not sufficient, however, to stabilize a political economy that faced great inherent strains after the First World War. If the defenders of interwar capitalism proposed a social bargain—the increasing satisfaction of material wants in return for a restoration of industrial authority—they had to be able to pay up. In the interwar period, however, many difficulties precluded paying up for more than a brief period.

Two interlocking flaws especially undermined sustained prosperity: constraints imposed by the international economy and by domestic conflicts. Once currencies were stabilized under the gold-exchange standard, balance-of-payment concerns, especially in light of the postwar creditor position of the United States, seemed to mandate relatively low European wages so that Britain and the Continent could maintain exports, compete internationally, and preserve their exchange rates. Reparation obligations for Germany and war debts for the Allies just made these constraints more demanding. At the same time, within each country, economic leaders remained preoccupied with potential saturation of the market and limits of profitability—what the Germans term *Rentabilität*. Industry spokesmen felt that profits were faltering, capital accumulation and investment was imperiled, and, in turn, international competitiveness endangered. They sharply attacked what they perceived as the politically determined costs of labor and of new social-insurance obligations.²⁹

But, while European businessmen fretted about impediments to accumulation, the relatively high rates of investment in the late 1920s may have outpaced the purchasing power that would sustain the return to capital. Although wages may not have lagged proportionally behind returns to capital,³⁰ urban and rural disposable incomes did not necessarily grow sufficiently to justify the continued “rationalization” of the 1920s. In formal terms, what had to be attained was a “warranted growth” path of capital and incomes that allowed the expansion of each to call forth and absorb the increments of the others. Only satisfying the

²⁸ Ernst Ulrich Huster, *Die Politik der SPD, 1945–1950* (Frankfurt a/M, 1978), 35–41. Also see Erich Pott-hoff, *Der Kampf um die Montanmitbestimmung* (Cologne, 1957); and Eberhard Schmidt, *Die verhinderte Neuordnung, 1945–1950* (Frankfurt a/M, 1970), 182–200.

²⁹ For the best recent discussion of these attitudes in Germany, see Bernd Weisbrod, *Schwerindustrie in der Weimarer Republik* (Wuppertal, 1978).

³⁰ Peter Temin has insisted on this, for the U.S.-European indices can be read in different ways; Temin, *Did Monetary Forces Cause the Great Depression?* (New York, 1976), 32.

two constraints together allowed each to be resolved in its own right. Only securing the two simultaneously, moreover, was likely to reconcile the major organized interest groups of the European economies.

Reading backward, one can say, of course, that the 1930s did not find the warranted growth path; and the original statements of the difficulty reflected the somber outcome in their pessimistic depiction of a "knife edge" that a dynamic economy had to tread if it was not to falter. Later theory, perhaps reflecting the generation of post-1950 growth, has suggested that in fact equilibrium growth is relatively easy to generate: technological substitutions, public spending, population growth, and income redistribution have all been shown to make ascent of the knife edge far less chancy.³¹ Indeed, the dilemma of equilibrium growth at the end of the 1920s was in part self-imposed by the reigning preoccupation with capital shortages and by the brakes placed on national income growth by the neomercantilist policies of the years following currency stabilization. There were dissenters to prevailing policy, such as John Maynard Keynes. But Keynes remained a gadfly and not always consistent in his recommendations. By the 1930s Keynes and like-minded adherents of purchasing-power doctrines pointed to the state as the agency that could assure high aggregate demand. Their intellectual task became simplified when they urged that their societies more or less disconnect from the international market and seek higher employment levels autarkically—that is, that they cease to fret about exchange rates. Indeed, abandonment of old currency parities followed almost by *force majeure* after 1931. In the long run, Keynes also felt, capital accumulation should become a less preoccupying task, for capital would become more plentiful in relation to the need for it.³²

Today these simplifying premises appear more problematic. Indeed, contemporary Western economic dilemmas suggest partial parallels with the difficulties perceived at the end of the 1920s. United States economic concepts for the post-war international economy largely precluded the welfare-state self-sufficiency that Keynes suggested. This meant further that his vision of satiated investment needs, with its resultant "euthanasia of the rentier," was likewise premature. Industrial societies in a world market arena can hardly allow investment to atrophy without losing real income to new competitors. Even to apply Keynesian macroeconomic stimulus to assure full employment may bring deteriorating balances of trade and, if no foreign subsidies are found, declining welfare. Some

³¹ For the "knife-edge view" of the warranted growth path, see R. F. Harrod, "An Essay in Dynamic Theory," *Economic Journal*, 49 (1939): 12–33, 377. Also see Evsey D. Domar, "Capital Expansion, Rate of Growth, and Employment," *Econometrica*, 14 (1946): 137–47. James Tobin and Robert M. Solow allowed for various tenable rates of growth with factor substitutability; see Tobin, "A Dynamic Aggregative Model," *Journal of Political Economy*, 63 (1955): 103–15; and Solow, "A Contribution to the Theory of Economic Growth," *Quarterly Journal of Economics*, 70 (1956): 65–94. For a recent optimistic summary that sees supply normally generating demand (with the 1930s as an exceptional catastrophe), see John Cornwall, *Growth and Stability in a Mature Economy* (London, 1972).

³² For Keynes's views concerning the decreasing scarcity of capital, see his *The General Theory of Employment, Interest, and Money* (London, 1960), 375–77; and, concerning the advantages of going it alone, see his *Essays in Persuasion* (1931; 2d ed., New York, 1963), 271–96, and "National Self-Sufficiency," *New Statesman and Nation*, July 8, 15, 1933.

of the constraints that vexed the 1920s have thus re-emerged and, with them, the distributive conflicts between the interests of wage earners and the spokesmen for capital. The difference is that, in the 1920s, the difficulties were rooted in too limited a confidence in mass consumption as a force for growth, whereas in the 1970s they may have derived from too excessive a reliance.

What remains historically remarkable is that from the late 1940s into the 1970s the constraints of the interwar period eased as a twin reorientation took place. First, the United States developed a commitment to European prosperity; second, the political and economic calculations of Europeans themselves changed so that they felt less locked into a distributive contest. Both changes together eased the iron framework of wages, profits, state claims, and international payments.³³

How could this reorientation take place so easily after 1945? For one thing, it was silently underway before that date. The Depression had certainly discredited the old orthodoxies. The war also demonstrated to British and American financial planners that states could impose levels of expenditure far beyond what the budget-balancers of the 1920s or British Treasury officials of the 1930s had imagined was safe and feasible.³⁴ Certainly the role of the United States was transformed: the credits of the 1920s had been extended via private banks and had remained hostage to the differential rates of return in Europe and New York; the grants of the late 1940s represented political decisions on the part of Washington. The new American policy did not come instantaneously or automatically. Just as between 1922 and 1924 the New York banking community accepted the need to intervene in Europe, so, as the newly opened records of the U.S. National Advisory Council on International Monetary and Financial Policies help show, Washington became increasingly willing to exploit foreign aid for political purposes: from the coy hesitation about extending loans to the Léon Blum mission in early 1946, to the vigorous European Recovery Program and the almost importuning support for noncommunist unions and parties by 1948, to the funds rushed to Yugoslavia after Tito's break with the Cominform.³⁵

The Marshall Plan signaled a political decision that the resources of the United States would be available for the reconstruction of a welfare capitalism in Europe. But in quantitative economic terms American aid amounted to little.

³³ The conflict between international competitiveness and demand stimulus at home has been brought out especially by the "Scandinavian" models of two-sector open economies. See Odd Aukrust, "Inflation in the Open Economy: A Norwegian Model," in Walter S. Salant and Lawrence B. Krause, eds., *Worldwide Inflation: Theory and Recent Experience* (Washington, 1977), 107-53; and Jeffrey Sachs, "Wages, Profits, and Macroeconomic Adjustment: A Comparative Study," *Brookings Papers on Economic Activity*, 2 (1979): 269-319.

³⁴ On the fiscal conservatism of the Treasury, see R. A. C. Parker, "Economics, Rearmament, and Foreign Policy: The United Kingdom before 1939—A Preliminary Study," *Journal of Contemporary History*, 10 (1975): 637-47; Robert Paul Shay, Jr., *British Rearmament in the Thirties: Profits and Politics* (Princeton, 1977), 73-79, 136-55, 242-46; and Susan Howson, *Domestic Monetary Management in Britain, 1919-38* (Cambridge, 1975), 120-26. For the transformation of attitudes, see Donald Winch, *Economics and Policy: A Historical Survey* (London, 1972), chap. 12; and Herbert Stein, *The Fiscal Revolution in America* (Chicago, 1969), chap. 8.

³⁵ Minutes of the Meetings of the National Advisory Council on International Monetary and Financial Policies, meetings 23, 24 (May 6, 1946), 89 (March 18, 1948), 112 (December 3, 1948, on Japan), 115-16 (January 7, 13, 1949), etc., Office of the Secretary of the Treasury, NA-RG 56.

For the major European economies from 1948 through 1951 it probably contributed no more than 10 to 20 percent of capital formation during the first two emergency years, then tapered off to below 10 percent.³⁶ Washington's assistance served more as capital-liberating than as capital-transfusing. "The basic elements in Western Europe's economic crisis . . .," the staff of the Eaton-Herter Select Committee on Foreign Aid accurately emphasized, "converge and appear in their most conspicuous aspect as a deficit in the balance of payments with the dollar area."³⁷ By easing balance-of-payments constraints and freeing key bottlenecks for specific goods, American aid allowed the European economies to generate their own capital more freely, certainly without returning to the deflationary competition of the 1930s. U.S. aid served, in a sense, like the lubricant in an engine—not the fuel—allowing a machine to run that would otherwise buckle and bind.

This calculation suggests that a modulated judgment on the role of American capital would be appropriate. Ultimately, the real sources of Europe's postwar

³⁶ The following table provides the ratio of grants and loans made by the the United States to the gross domestic capital formation of the respective countries. Gross domestic capital formation (converted here into dollars at current exchange rates) is a more relevant measure for the postwar years than net investment, for the replacement of depreciated plants meant qualitative improvement. (For Italy in 1948 and 1949, only net figures are available.) Grants extended during 1948 comprised largely "interim aid" as a stop-gap before Marshall Plan funds strictly speaking came on stream. After 1951, Marshall Plan aid was phased into Mutual Security assistance with major military components.

Country	1948	1949	1950	1951
UNITED KINGDOM				
U.S. Aid	\$ 937m	\$1,009m	\$ 629m	\$ 129m
GDCF in \$	\$10,400m = 9%	\$9,000m = 11%	\$6,400m = 10%	\$6,300m = 2%
FRANCE				
U.S. Aid	\$ 781m	\$ 766m	\$ 465m	\$ 421m
GDCF in \$	\$ 5,600m = 14%	\$6,400m = 12%	\$4,460m = 10%	\$5,380m = 7%
WEST GERMANY				
U.S. Aid	\$ 1,130m	\$ 948m	\$ 470m	\$ 362m
GDCF in \$	\$ 3,600m = 31% (est.)	\$4,340m = 22%	\$4,400m = 11%	\$5,300m = 7%
ITALY				
U.S. Aid	\$ 399m	\$ 437m	\$ 257m	\$ 261m
GDCF in \$	\$ 1,500m = 27% (net)	\$1,300m = 34% (net)	\$2,700m = 10%	\$3,000m = 9%

NOTE: All figures in millions of current (1948–51) dollars; only net figures are available for Italy in 1948 and 1949, and only an estimate can be made for West Germany in 1948, since the available statistics do not give figures for the first half of that year.

SOURCES: Totals of American aid have been taken from *Statistical Abstract of the United States*, 1954, 898–902; British GDCF, from *Statistical Abstract for the United Kingdom*, no. 87 (1938–49): Table 294, no. 88 (1950): Table 296, and no. 89 (1952): Table 288; and, other 1948–49 statistics, from *Statistisches Jahrbuch für die Bundesrepublik Deutschland*, 1952, 454–55; *Annuaire Statistique de la France*, 59 (1952): 335; and *Annuario Statistico Italiano*, ser. V, 3 (1951): 590. Non-British GDCF estimates for 1950 and 1951 are taken from United Nations, *Yearbook of National Accounts Statistics* (1957).

³⁷ U.S. Congress, House Select Committee on Foreign Aid, *Final Report on Foreign Aid* (May 1, 1948), 80th Cong., 2d sess., House Report no. 1845, p. 24.

growth had to derive from the Continent's own energies. Indeed, some recovery was apparently already underway by late 1946, even for the battered West German economy.³⁸ Had not the fearsome winter of 1946–47 paralyzed transportation, impeded food and fuel deliveries, and radicalized workers into politically explosive wage demands, recovery might have continued. In that case, without the emergency American response the ongoing European economic performance might well have resembled, say, British growth in the late 1930s: more protectionist and less spectacular than was to be racked up under American auspices in the 1950s, but still respectable.

In this regard, the American economic role in restabilization after World War II paralleled the political role. Europe would probably not have “gone Communist” or collectivist even if the United States had not intervened with the same resolution. The European middle classes remained socially anchored; the German occupation had hardly struck or aimed at them as a group, nor had it attacked their economic values. But both the political and economic development of the 1950s would doubtless have been less resolutely capitalist and market-oriented, less justified by dynamic success. Throughout the first three postwar years, in fact, there was less decisive purpose than confused experimentation and uncertain initiatives. Business recovery was not held back by ideological sympathies for socialism but by the fear of risky venture, the hesitation finally to write off the losses of the war years. Between 1945 and late 1947, for example, the French and the Italians, then the West Germans along with their American occupiers, avoided imposing the deflationary reforms that helped invigorate capitalist growth.³⁹ Nor were they prepared to abandon the fuzzy political compromises, which found expression in the tripartite Catholic-Socialist-Communist governing coalitions but seemed less and less likely to mandate either socialism or renewed capitalist growth. Only in 1947–48, when ideological and economic threats appeared potentially catastrophic, did the spokesmen for West Europe's middle classes and elites, and their American sponsors, resolve upon the liberal capitalist mandate that might best be described as a new “wager upon the strong.”

Economic analysts have proposed several theories for the remarkable growth that followed. Structural explanations include the sharp increase in agricultural productivity achieved by tractors and fertilizers, the resultant supply of labor released for industry (a supply already augmented by the migrants from eastern Germany and the Italian South, among other areas), and the special efficiency

³⁸ Werner Abelshauser, *Wirtschaft in Westdeutschland, 1945–1948* (Stuttgart, 1975), 167–70.

³⁹ For the French rejection of deflation, see Richard Kuisel, *Modernization and the Managed Economy: The State and Capitalism in France, 1900–1950* (Cambridge University Press, forthcoming), chap. 7. For the Italians, who in 1947 embarked upon deflation, see George H. Hildebrand, *Growth and Structure in the Economy of Modern Italy* (Cambridge, Mass., 1965), chaps. 2, 8; Marcello De Cecco, “Sulla politica di stabilizzazione del 1948,” in his *Saggi di politica monetaria* (Milan, 1968), 109–41; and Camillo Daneo, *La politica economica della ricostruzione, 1945–1949* (Turin, 1975), chap. 7. On the American and German hesitation to impose early currency reform, see Edward A. Tennenbaum, “The German Mark,” book draft, chaps. 11–12, Tennenbaum Papers, box 3, folder 5, Truman Library, Independence, Mo. Belgium was the outstanding exception to the general inflationary languor at the end of the war. For the reforms of Camille Gutt, see Léon H. Dupriez, *Monetary Reconstruction in Belgium* (New York, 1947).

of investment in the context of postwar damage and renewal. Monetarist accounts attribute success to rigorous stabilization programs in Germany, Italy, and Japan. The historian can point to the wage restraint that Dutch, German, and Italian workers demonstrated because of labor's commitment to reconstruction and, perhaps, to mere exhaustion after fascist repression and war.⁴⁰ On the managerial side, new business confidence and technocratic impulses gradually prevailed. The example of Pont-à-Mousson suggests that once public policy-makers, such as Jean Monnet or Robert Schuman, made commitments to supranational institutions, a new generation of expansionist entrepreneurs could find support for pressing vigorous investment plans within their own firms.⁴¹

The upshot was that both the major restraints that had corseted the economy of the 1920s could be loosened together. U.S. aid helped overcome the deflationary pressures resulting from defense of the balance of payments. But these pressures also remained minimal because a new generation of Keynesian-influenced administrators were willing to take international deficits in stride. Establishment of European-wide clearance schemes and the willingness of intra-European creditor countries, such as Belgium and even Italy, to hold sterling or other European currencies as a *quid pro quo* for American aid also eased the strains on the economies tending toward balance-of-payments deficits. Washington policy-makers certainly did not like the impediments to currency convertibility that Europeans kept in force, and they continued to press for the removal of these obstacles to the free circulation of dollars. U.S. Treasury officials and American delegates to the International Monetary Fund insisted stubbornly on convertibility even at the cost of deflationary policies. In contrast, American officials with the Marshall Plan administration (the ECA) tended to accept compromise arrangements that permitted Europeans to prolong shielding their international accounts; and even the stern Treasury disciplinarians had to accept British cancellation of sterling convertibility after the disastrous attempt during the summer of 1947. They likewise were compelled to acquiesce in French creation of a two-tiered currency market in 1948, which allowed scope for floating exchange rates, and they accepted restrictions upon full convertibility in the European clearance unions from 1949 through 1951. Preaching that all currencies should be fully tradable for dollars, Washington officials nonetheless lived with a compromise monetary regime.⁴²

⁴⁰ For examples of structural approaches, see Ingvar Svennilson, *Growth and Stagnation in the European Economy* (Geneva, 1954); U.N. Economic Commission for Europe, *Economic Survey of Europe in 1961*, part 2: *Some Factors in Economic Growth in Europe during the 1950's* (Geneva, 1961); and Charles Kindleberger, *Europe's Postwar Economic Growth: The Role of Labor Supply* (Cambridge, Mass., 1967). For an example of the monetarist approach, see Hildebrand, *Growth and Structure in the Economy of Modern Italy*. Angus Maddison has emphasized policy factors, including a Western internationalism attributed to the Cold War; see his "Economic Policy and Performance in Europe, 1913-1970," in Carlo Cipolla, ed., *The Fontana Economic History of Europe*, 5 (Glasgow, 1976): 442-508. For a general treatment, see M. M. Postan, *An Economic History of Western Europe, 1945-1964* (London, 1967).

⁴¹ See, for example, the debate on expansion of coking facilities and Roger Martin's advocacy of investment, October 16, 1951, PAM, box 70671. Also see Richard Kuisel, "Technocrats and Public Policy: From the Third to the Fourth Republic," *Journal of European Economic History*, 2 (1973): 53-99.

⁴² For debates on convertibility, see Minutes of the Meetings of the National Advisory Committee on Inter-

The second major inhibition that had undermined continuing expansion in the 1920s also disappeared: the precarious "knife edge" equilibrium growth path for wages and investment broadened into an easy highway. If policy-makers no longer wished to sacrifice living standards on the altar of fixed exchange rates, labor showed sufficient wage restraint such that investment could soar. Rather than relatively high labor costs impelling capital substitution, relatively low labor costs permitted capital expansion. The statistics of the 1950s reveal not only the familiar growth of national income but unprecedented rates of capital formation as well: 30 percent in Japan, 27 percent in Germany, 20 percent in France and Italy, 16 percent in the United Kingdom, 18 percent in the United States.⁴³ In contrast, the wages share of national income remained stable or even dropped slightly, as in Western Germany: a decade's halting of the slow but otherwise prevailing trend of the twentieth century. The expansion and harmony that businessmen had sought in the 1920s was finally achieved in the 1950s.

This result, of course, required the cooperation of those labor leaders who shared the premises of a growth-organized welfare capitalism—the commitment that I have elsewhere termed the "politics of productivity."⁴⁴ "The improvement of productivity, in its widest sense, remains the fundamental problem of Western Europe," declared the Organization for European Economic Cooperation, and it echoed the themes of the managerial mystique of the 1920s as it reported, "Great emphasis is placed in the United States . . . upon public relations efforts by management in acquainting workers with their plant, its problems, and its place in the economy."⁴⁵ For society as a whole, the politics of productivity meant simply the adjournment of conflicts over the percentage share of national income for the rewards of future economic growth. As one West German official explained to business and labor representatives in the remarkable Königstein discussions of February 1949 (which, in effect, adumbrated West German economic strategies up to the present day), anyone who could renounce some consumption had to renounce it. "He had to save, whether or not he wanted, because he cannot be permitted to evade the common tasks of reconstruction."⁴⁶

national Monetary and Financial Policies, meetings 70, 79–81, 83–84, 134, 153, 158, 171, Office of the Secretary of the Treasury, NA-RG 56. For the EPU, see William Diebold, Jr., *Trade and Payments in Western Europe* (New York, 1972), 64–69; and Albert O. Hirschman, "The European Payments Union: The Negotiations and the Issues," *Review of Economics and Statistics*, 33 (1951): 49–59.

⁴³ Simon Kuznets, *Modern Economic Growth: Rate, Structure, and Spread* (New Haven, 1966), 236–37; and U.N. Economic Commission for Europe, *Some Factors in Economic Growth in Europe during the 1950's*, chap. 2, pp. 16–22. For the wage share of national income, see U.N. Economic Commission for Europe, *Incomes in Postwar Europe: A Study of Policies, Growth, and Distribution* (Geneva, 1967), chap. 2, pp. 30–31.

⁴⁴ See my "The Politics of Productivity: Foundations of American International Economic Policy after World War II," in Peter Katzenstein, ed., *Between Power and Plenty: The Foreign Economic Policies of Advanced Industrial States* (Madison, Wisc., 1978), 23–49; the articles in this volume were first published in 1977, as the autumn issue of *International Organization*.

⁴⁵ Organization for European Economic Co-Operation [hereafter, OEEC], *Europe: The Way Ahead: Towards Economic Expansion and Dollar Balance*, 4th Annual Report of the OEEC (Paris, 1952), 195. Also see Roger Grégoire, "European Productivity Agency," in OEEC, *At Work for Europe* (5th ed., Paris, 1960), 139–52.

⁴⁶ Statement of Dr. Troeger, Königstein, January 4, 1949, BA Koblenz, Z 13/63. The labor minister of the Bizone, Halbfell, dissented, arguing against unplanned investment, but was in a clear minority.

As an explicit principle of consensus, economic growth—the notion of continuously higher levels of national product—came into its own at the end of the 1940s. The earliest public celebration of its virtues may well have been Leon Keyserling's speeches as chairman of the Council of Economic Advisers during 1949;⁴⁷ but the less precise concepts of sustained high purchasing power or simply "reconstruction" or "production" served to rally labor as well as businessmen from the end of the war on.

Throughout 1945 and 1946 Communist labor leaders themselves seemed ready to accept the trade-off between present consumption and future growth. The increasing hardship of their rank and file during the winter of 1946–47 and the threat of militant unions on their left flank (aside from any guidance that Moscow may have urged as the dispute with the United States deepened) impelled them to abandon their collaborative stance. The French Communists' reluctant sponsorship of the Renault strike, which likewise led to their dismissal from the governing coalition (and, similarly, the Belgian Communists' refusal to accept coal price increases), best revealed their shifting priorities. No less anti-communist an AFL representative than Irving Brown, who felt that the successive strikes revealed the Communists' "complete desire to destroy the government even at the cost of permanently destroying France," understood that a socialist movement could hardly recapture leadership within the CGT if it participated in a cabinet seeking to freeze wages.⁴⁸ Despite the admitted difficulty in reconstructing a mass base for the socialists, by 1947–48, American policymakers, AFL emissaries, and European businessmen diligently encouraged the formation of social democratic unions in the Latin countries and pressed for the purge of Communist sympathizers from British, German, and American federations. The moderates of Force Ouvrière, the TUC, or the Italian Catholic union federation (CISL) became all the more essential as interlocutors for labor. "The trend in Europe is clearly toward the Left," noted one of the Department of State's leading European analysts shortly after tripartism collapsed. "I feel that we should try to keep it a non-communist Left and should support Social-Democratic governments."⁴⁹ The axis of the politics of productivity thus had to fall

⁴⁷ Keyserling, "Prospects for American Economic Growth," Address in San Francisco, September 18, 1949, Truman Library, President's Secretary's File 143: "Agencies: Council of Economic Advisers."

⁴⁸ Brown, "Report on Greece, France, and England," July 7, 1947, State Historical Society of Wisconsin, AFL Papers, Florence Thorne Collection, 117/8A, box 17, F. 3A. On the events of 1947, see Wilfried Loth, "Frankreichs Kommunisten und der Beginn des kalten Krieges: Die Entlassung der kommunistischen Minister im Mai 1947," *Vierteljahrshäfte für Zeitgeschichte*, 26 (1978): 9–65, and "Die französischen Sozialisten und der Marshall-Plan," in Lutz Niethammer, ed., *Die europäischen Linke und der Marshall-Plan* (forthcoming). Also see Vincent Auriol, *Journal du Septennat*, ed. Pierre Nora and Jacques Ozouf, volume 1: 1947 (Paris, 1970), *passim*; and Alfred Rieber, *Stalin and the French Communist Party, 1941–1947* (New York, 1962), 331–57. On Belgium, see NA-RG 59, 855.00/3-1147 (no. 372), 855.00/3-2147 (no. 1069), 855.00/3-3147 (no. 1097).

⁴⁹ John Hickerson to H. Freeman Matthews, June 25, 1947, NA-RG 59, Office of European Affairs, box 3. For European policies of the AFL, see International Labor Committee, Minutes of the Meeting of November 11, 1947, AFL Papers, Florence Thorne Collection, 117/8A, box 17, F. 3C. Also see Matthew Woll to Thorne, April 6, 1948, and the attached "Confidential Report," *ibid.*, F. 4. And see Ronald Radosh, *American Labor and U.S. Foreign Policy* (New York, 1969); Ulrich Borsdorf, "Erkaufte Spaltung: Der Marshall-Plan und die Auseinandersetzung um die deutschen Gewerkschaften," in Niethammer, *Die europäischen Linke und der Marshall-Plan*; Horst Lademacher, "Die Spaltung des Weltgewerkschaftsfundes als Folge des beginnenden Ost-West Konfliktes," *ibid.*; and Lutz Niethammer, "Strukturreform und Wachstumspakt," in Heinz Oskar Vetter, ed., *Vom*

right in the center of the labor movement: "politically speaking the break must come to left of or at the very least in the middle of the [French] Socialist party. Translated into labor terms, the healthy elements of organized labor must be kept in the non-Communist camp. Otherwise the tiny production margin of the fragile French economy would vanish and the ensuing civil disturbances would take on the aspects of civil war."⁵⁰

The economic premises that the "healthy elements" of labor subscribed to remained precisely those of the trade-union leaders who had pioneered collaborative labor relations in the late 1920s. Union spokesmen such as Ernest Bevin had then joined progressive industrialists for talks on enhancing productivity. By the late 1940s they were serving in high office. Their integration testified to the post-war years' fulfilment of the second criterion for stabilizing the welfare capitalist economies of the West. The new cooperation, along with America's underwriting, ensured that capital accumulation and wages and welfare benefits could increase in tandem, thus overcoming the fatal impediments to sustained growth in the 1920s. As Western leaders looked more and more to economic growth, increasingly presupposed, first, as automatic and, second, as the major index of a society's welfare, the stakes of politics narrowed. Communism increasingly became a permanent and sullen opposition, to be analyzed, in the spirit of the 1950s, as inherently pathological. At the same time, the appeal of neofascism or Gaullism remained fitful, largely consigned to the regions that paid for dynamic growth elsewhere with their own relative backwardness. In the political center Christian Democrats (or Tories in Britain) either shared power with Social Democrats or alternated officeholding in a consensual politics that debated only whether the anticipated dividends of economic growth should be devoted to social-welfare consumption or ploughed back into private investment. Residual colonial or religious and ethnic issues—not the baselines of political economy—remained the major sources of passion and controversy.

Repression, cooptation, and the success of the managerial mystique with its vogue of productivity had reconsolidated the bureaucratic organization of industrial work in the 1920s. The economic accomplishments of the period after 1948 completed the second half of the stabilization assignment. They seemed to eliminate the vulnerability of economic life and enhanced legitimacy with output and growth. Despite the tragic waste of the Great Depression, the immense destructiveness of two world wars, and the countless lives scattered like dry autumn leaves throughout Europe, Western leaders recovered more of their prosperity and liberalism, retained more of their privileges and prerogatives, than they would have dared predict.

Sozialistengesetz zur Mitbestimmung: Zum 100. Geburtstag von Hans Böckler (Cologne, 1975), 303–58. On the French unions, see Bergonieux, *Force Ouvrière*; and André Barjonet, *La C.G.T.* (Paris, 1968), 49–51. On Italy, see Daniel L. Horowitz, *The Italian Labor Movement* (Cambridge, Mass., 1963), 208–73; and Adolfo Pepe, "La CGIL dalla ricostruzione alla scissione, 1944–1948," *Storia Contemporanea*, 5 (1974): 591–636, and the works cited in n. 31. Also see the reports from Paris and Rome to the Department of State in *Foreign Relations of the United States*, 1947, 3 (Washington, 1972): 690–91, 695–99 (on the CGT), 847–48, 863–68 (on Italian labor).

⁵⁰ Robert Lovett to Ambassador Caffrey, Paris (based on a memo by Hickerson), October 25, 1947, NA-RG 59, 851.00/10-2447.

SUCCESSFUL SYSTEMS OF POLITICAL EQUILIBRIUM must remain isolated (as did Tokugawa Japan) or be international in scope. The notable eras of European stabilization—the generation after Utrecht, for example, or the half-century after Vienna—have been periods of class equilibrium and international compromise simultaneously. The configurations of power among states tend to second those within societies. The Vienna settlement consisted of adjustments between states but also comprised a restoration of old and new landed classes along with a strengthened bureaucracy. *Pax Britannica* assimilated bourgeois elements to this international coalition and added resources outside Europe to equilibrate strains at home. Fully to comprehend the period from 1918 to 1950 as a search for stabilization on the part of old upper and middle classes, now augmented by a reformist working-class leadership, requires looking at the international architecture as well as domestic structures. Obviously, the Cold War had a decided influence on internal outcomes after World War II. But to register this connection hardly reveals the principles of interaction. The Cold War did not, in itself, determine the logic of the international system for domestic stability.

The surprising centers of growth in the 1950s and 1960s were West Germany, Japan, and, though a smaller economy, Italy. West Germany and Japan, above all, became virtual engines of capital accumulation. As such, they played a critical role in U.S. encouragement of an international coalition of liberal polities with mixed capitalist economies. Although, as of 1944, the U.S. Treasury resoundingly rejected the idea that a German economic contribution would be vital for European prosperity, Congressmen, the Harriman mission (to prepare for Marshall Plan aid), and industrial leaders by 1947 viewed German recovery as doubly critical, both for its own sake and for the economic linchpinning of the wider region.⁵¹ If integrated into a West European system of exchange, German skilled labor, technological virtuosity, and coal would benefit all her neighbors. Without German recovery and integration, their economies must operate less efficiently. The same calculation came to hold for Japan and its role in America after the Communist takeover in China and hostilities in Korea.⁵² Opponents of a punitive treatment for Germany had emphasized their European economic vision from the outset, and by the summer of 1947 their concept had quickly become the main theme of the influential spokesmen for German recovery. The lesson was not lost on industrial interests in the emerging state: when German firms petitioned to raise their output or rebuild their rolling mills, their directors

⁵¹ U.S. Treasury Memorandum, "Is European Prosperity Dependent upon German Industry?" September 7, 1944, Mudd Library, Princeton University, Harry Dexter White Papers, box 7, F. 22: "In short, the statement that a healthy European economy is dependent upon German industry was never true, nor will it be true in the future." For the turnabout, see "Records of Conferences," Harriman mission, summer 1947, W. A. Harriman's papers, Washington, D.C. Also see John Gimbel, *The Origins of the Marshall Plan* (Stanford, 1976), and *The American Occupation of Germany, 1945-1949* (Stanford, 1968), 147-58, 163-69, 174-85.

⁵² Joyce Kolko and Gabriel Kolko, *The Limits of Power: The World and United States Foreign Policy, 1945-1954* (New York, 1972), chaps. 11, 19; Jon Halliday, *A Political History of Japanese Capitalism* (New York, 1975), 182-90; and John Dower, *Aftermath of Empire: Yoshida Shigeru and the Japanese Experience, 1878-1954* (Cambridge, Mass., 1979), chaps. 9-10, and manuscript essays on "The Reverse Course."

unabashedly pleaded the cause of good Europeans.⁵³ Nor was recovery only the demand of businessmen. German trade unions and the AFL, which supported them, strongly advocated industrial reconstruction.⁵⁴ Rehabilitation of the German economy thus emerged as critical for the United States's wager on productivity.

Was it just an accident that the countries that forged ahead so brilliantly and then came to serve as international poles of growth even beyond expectation were the exfascist powers? This question must be confronted, despite its harsh implications. Did Washington, in effect, reap the final benefit from the discipline and coercion of labor that the Axis states had earlier imposed? Not directly, of course. But the American-sponsored international economy may have ultimately benefited from the fact that the working classes within the defeated countries had been atomized by political repression, wartime sacrifices, and the mere tasks of survival. Labor leaders who returned from concealment, prison, or exile faced sufficient challenge just in rebuilding their shattered movements. Stressing the necessity of production appeared to them less a contribution to restoration than the premise for the patient work of reorganization.⁵⁵ In addition, defeat and occupation clearly permitted the United States more direct intervention than was possible elsewhere. Occupation authorities in all three countries could limit the organization of political unions, postpone nationalization, and halt strikes. Allied fiscal control—exerted perhaps most consistently by Joseph Dodge in Japan⁵⁶—ultimately reinforced those who advocated rapid capital formation, although businessmen often resisted at first. Harder to measure, but just as important, was the yearning for private goals in countries where fascists had sought to politicize all aspirations and relationships. The United States, after all, was gambling on the renewed persuasiveness of individual well-being.

Germany, however, had hitherto repeatedly resisted integration into an international productive coalition. Insofar as the international divisions of the period from 1914 to 1950 had an economic dimension, they involved conflict less between capitalist societies and a Bolshevik challenger than among different capitalist alternatives. Anglo-American disagreements over the organization of a global economy persisted and raised bitter recriminations on each side. The issue remained whether the international economy should maximize multilateral

⁵³ See, for example, Akten des Verwaltungsamtes für Eisen und Stahl, BA Koblenz, Z 41/23: "Vorschlag zur Wiedereinschaltung der August Thyssen Hütte in der europäischen Stahlplanung . . . 9 Februar 1950."

⁵⁴ For example, see the works council of Robert Bosch, AG's protest against decartelization proceedings, March 17, 1948, Deutscher Gewerkschaftsbund Archiv, Düsseldorf: "Wirtschaftspolitik, Dekartellierung 1948-49." For similar objections to controls on German industry, see BA Koblenz, B 109/345: "Stellungnahme der Gewerkschaften zum Ruhrstatut vom 7. Januar 1949." For a specimen of AFL support, see William Green to President Truman, November 24, 1947, AFL Papers, William Green Collection, 117A/11C, box 7F (Marshall Plan).

⁵⁵ For an example of this organizational effort, see [Hans Böckler] "Bericht der Deutschlandreise, 6. März bis 30. April 1946," Deutscher Gewerkschaftsbund Archiv, Düsseldorf.

⁵⁶ Detroit Public Library, Joseph Dodge Papers, Japan Assignment, box 1, F: "Budget: Ikeda Interviews," and Japan Assignment, 1950, box 3, F: "Correspondence, Marquat."

trade and welfare, but thereby reward the most massive and technologically productive economy, or whether as the British desired, it should be based upon regional systems of dominion that guaranteed international markets to the weaker power.⁵⁷ Still, the British dominion alternative seemed to be a limited challenge, whereas the German threat to the open international economy had been more ominous and, just as critical, the emanation of an ugly political regime.

This is not to argue that Nazism was menacing because of its international economic policies—the autarky and bilateralism that so angered Cordell Hull. Instead, the connection between politics and economics was central to the very way Nazism was interpreted as a regime. American commentators viewed Nazism as an abusive political economy: a cartel of monopolists who subordinated the public sphere to private forces.⁵⁸ Although Hjalmar Schacht's bilateral treaties yoked Eastern Europe into a German-dominated economic bloc, trade access to this area was hardly a crucial stake in itself. Nevertheless, a Germany that was enrolled in a system of international exchange with the West, as the Weimar Republic had been from 1924 to the Depression,⁵⁹ naturally appeared a safer and more decent participant in a liberal international order.

Hence the central conflict defining the international political economy from World War I until about 1950 was not that between American and Soviet alternatives, between capitalism and communism. The Soviet-American antagonism after World War II, in effect, imposed a framework on international politics but did not exhaust the issues. Viewed over the whole half century, the American international economic effort of the era of stabilization centered on overcoming British, Japanese, and especially German alternatives to a pluralist, market-economy liberalism. In the case of Germany, these alternatives were incorporated first in Berlin's vision of Mitteleuropa during Ludendorff's regime of 1917–18 and then in Hitler's expansionist Reich. Defeating these German projects, however, could be only the first stage in erecting a stable alternative. To assure liberal, pluralist stability within each West European country, as well as for the Atlantic region as a whole, required the further step of integrating German economic dynamism into an international system of exchange: perhaps the pre-eminent Western diplomatic task in each postwar reconstruction period.

These respective postwar tasks, however, took more than just German defeat; they also required that the United States assume the burden of funding Germany's international deficits—including reparations—after the two wars. Amer-

⁵⁷ Richard Gardner, *Sterling-Dollar Diplomacy: Anglo-American Cooperation in the Reconstruction of Multilateral Trade* (Oxford, 1956); and Benjamin M. Rowland, "Preparing the American Ascendancy: The Transfer of Economic Power from Britain to the United States," in Rowland, ed., *Balance of Power or Hegemony: The Interwar Monetary System* (New York, 1976), 195–224.

⁵⁸ Maier, "The Politics of Productivity," 32–34. Roosevelt's own message calling for the Temporary National Economic Committee investigation on monopoly, April 29, 1938, defined fascism as "ownership of government by an individual, by a group, or by any other controlling private power," an interpretation that linked the attitudes of the "second" New Deal with the concern about Nazi expansionism.

⁵⁹ Werner Link, *Die amerikanische Stabilisierungspolitik in Deutschland, 1921–1932* (Düsseldorf, 1970); and Gerd Hardach, *Weltmarktorientierung und relative Stagnation: Währungspolitik in Deutschland, 1924–1931* (Berlin, 1976), 152–62.

ican reluctance to take on this responsibility until 1924 (and then only indirectly) helped produce the impasses of the five years after Versailles. U.S. willingness to take on the burden after 1947 facilitated the stabilization of the 1950s and 1960s. But American readiness was no automatic decision. As one minor Department of State official wrote before victory in Europe, "It seems certain that Germany has lost the war; but it appears that Dr. Schacht has a very good chance of winning the peace."⁶⁰

In light of these developments, the international corollary of the era of domestic stabilization may be viewed as a German-American (or perhaps a trilateral German-American-Japanese) association achieved only after two world wars. Success for this policy was registered not by the rubble of Berlin but by the frustration of such postwar German leaders as Jakob Kaiser of the CDU and Kurt Schumacher of the SPD, both of whom sought unsuccessfully to maintain under democratic auspices a less capitalist and less exclusively Western-oriented German society.⁶¹ Their very setbacks testified to the triumph of stabilization in West Germany, Western Europe, and the noncommunist countries as a whole. Just as the end of the second war against Germany resolved the international issues left undecided after the close of the first, so the strengthening of Western pluralism after the second war completed the European domestic institutional restructuring begun after the first. Stabilization meant an end to the German problem. It likewise meant winning the adherence of a large enough segment of the working classes to preserve the scope for private economic power and hierarchy that defined liberal capitalism. The achievement was not simply restorative, for the new, very real guarantees of social welfare and social-democratic political participation contributed change even as they purchased continuity.

This suggests that the major sociopolitical assignment of the twentieth century paralleled that of the nineteenth, which saw the incorporation of the middle classes and European bourgeoisie into the political community. The international corollaries of the earlier development were the paralysis and reduction of Metternichian Austria within Europe and the extension of overseas empire. The international corollaries of the new development were the linking to the West of at least part of Germany and the recession of overseas empire: the trajectory from grandeur to welfare. The institutional device for the nineteenth century was parliamentary representation; the institutional foci for the twentieth-century achievement included trade unions, ambitious state economic agencies, and bureaucratized pressure groups—the components of what I have termed elsewhere "corporate pluralism."

Observers have often failed to note the magnitude of the twentieth-century

⁶⁰ Joseph Fuqua to Woodrow Willoughby, December 21, 1944, NA-RG 59, International Trade Papers, box 19, F: "Article VII: United Kingdom—General."

⁶¹ Hans Peter Schwartz, *Vom Reich zur Bundesrepublik: Deutschland im Widerstreit der aussenpolitischen Konzeptionen in den Jahren der Besatzungsherrschaft, 1945–1949* (Berlin, 1966), 297–344; Werner Conze, *Jakob Kaiser: Politiker zwischen Ost und West, 1945–1949* (Stuttgart, 1969); Lewis J. Edinger, *Kurt Schumacher: A Study in Personality and Political Behavior* (Stanford, 1965); and Ernst Nolte, *Deutschland und der Kalte Krieg* (Munich, 1974), 208–14, 322–23.

accomplishment because the costs were so distressing. Certainly this essay should not be read as an argument that, because stability resulted, the intervening tyranny, warfare, sacrifice, and resistance lose their historical significance. Still, to ask about significance is to search for meaning, which is just one task of history. To trace the structural principles of collective life must remain an equally valid historical enterprise; and that pursuit compels us to admit that even catastrophic events do not always durably alter the trajectory of institutions any more than the constant slow renewal that precedes in the absence of disaster. Indeed, that continuing change best facilitates the analysis of earlier patterns. If now the institutional solutions of the second postwar era show signs of wear and tear, if the social compromises of the welfare state become precarious as economic growth falters, if the stability of the past generation appears perhaps to have rested on exceptional and transitory advantages, such as the consensus on postwar reconstruction or the ease of securing resources from outside Europe, then we can better begin to understand the recent era not merely as events but as history.

Comments:

THE HISTORIAN WHO SEEKS TO DEVELOP AN INTERPRETIVE FRAMEWORK for elucidating twentieth-century social and economic problems encounters formidable obstacles. Modern societies, unlike Gothic cathedrals, are pluralistic structures. They rarely involve the combined efforts of men laboring over a sustained period in such a way that a skilled analyst can show them progressing, despite variations in detail, toward a common goal. Hence, the narrow focus of most writing on contemporary historical topics reflects the actual fragmentation of industrial society. All the same, even professionals in the field feel a profound need for synthesis. The document-bound monographs—so runs the complaint—tell us more and more about less and less. We hunger for the perspective afforded in other segments of the discipline by discussion of *la longue durée*. How, then, should the aspiring generalist proceed?

Two strategies figure as possibilities. The first commends itself to those possessed of a cautious nature but not easily fatigued. It requires replicating, at least selectively, the primary research of the monograph writer as well as examining systematically the secondary literature. Generalizations emerge from the data in inductive fashion and are therefore likely to prove reliable. Unfortunately, archival research on a massive scale becomes prohibitively expensive, and exhaustive scrutiny of the existing works for an extended chronological period can push even a dedicated student to the limits of endurance. What is more, the risk remains that the result will echo the specialized writer's positivist inclinations rather than produce an original synthesis. An alternative strategy thus appears initially more promising. One begins with a governing hypothesis and then surveys the literature selectively to find substantiation for it. If the hypothesis is shrewdly chosen, the outcome may suggest an enlightening way to organize information within a plausible explanatory structure. That strategy, however, also harbors dangers. It provides little incentive to avoid the use of unrepresentative data. And it can tempt a writer into employing abstract terms in order to obscure the discrepancies between an arresting hypothesis and inconvenient facts.

PROFESSOR CHARLES S. MAIER HAS ADOPTED THE SECOND STRATEGY in his ambitious discussion of Western Europe's path to political and social stability. His interpretation bears witness both to the merits and the disadvantages of the genre. Since Maier's diction is at times elusive, it may prove useful to recapitulate received orthodoxy and to contrast it with what I take to be his views. Conventional wisdom holds that Western Europe's bourgeois elites managed after World War II to sustain their parliamentary leadership and to preserve the es-

sence of market-oriented economies for four relatively straightforward reasons. The forces of the Left, despite the prestige garnered in Resistance activity, possessed neither the requisite economic skills nor a realistic vision for restoring shattered national economies to productivity. The Christian Democratic parties, notwithstanding a rhetorical commitment to social reform, rapidly became catch-all groupings that absorbed nearly everyone else in the political class, including the old elites who monopolized managerial talent. Moreover, the mass of people did not want revolution. Whatever hopes left-wing intellectuals might indulge, workers and peasants (where given a choice) preferred to join in the benefits of bourgeois society rather than to destroy it. Finally, the threat of Soviet imperialism divided the moderate Left from the Communist leadership and, at the same time, led the United States—for a mixture of security, commercial, and humanitarian motives—to underwrite European reconstruction during a crucial transition period. The Marshall Plan administrators would have preferred, on balance, to manipulate aid in order to advance the cause of social democracy, but a sufficiently powerful democratic Left existed for this nowhere on the Continent. Hence, it is no surprise that reconstruction came under traditional bourgeois auspices. The most successful economic recoveries—in Germany and in Italy—were engineered by financially orthodox, free-market liberals. Only in England did Labour prevail. There, the burdens of the welfare state, climaxing a wartime increase in the wage earner's share of national income, diverted resources from capital formation and retarded economic growth—to the eventual detriment of the whole society.

This account, in Professor Maier's eyes, is too narrowly focused and lacks a theoretical dimension. Both Left and Right, he claims, expected "radical economic and social change" after each of the world wars (page 328). Such change failed to materialize because of a "single half-century effort" by elites who made clever compromises to maintain the capitalist system (page 328). What is capitalism? As Maier defines it, capitalism is not simply a means of production, distribution, and marketing. It is rather an overlapping hierarchy of power, wealth, and status. In fact, it comes perilously close to what others might describe as bourgeois democracy. Maier asserts that universal suffrage necessarily brought the "legitimacy" of the traditional hierarchy into question. The capitalist classes had to elaborate a defense for the deployment and compensation of managerial skills, or, as he puts it, a justification why "only small minorities could share the prerogative of directing human labor" (page 333). After several decades of fumbling, they did so by embracing Keynesian economics and instituting the welfare state. Planners in the 1920s had found themselves bedeviled by twin difficulties: wages and social benefits that were inflated for political reasons, and markets that were saturated owing to underconsumption. A generation later, they discovered a way to escape from the old ideological conflicts over distribution of income and wealth by identifying the "warranted growth path" and unlocking the secrets of rapid economic progress. Maier discerns, by the late 1940s, an increasing collaboration of business and labor for "growth-organized welfare capitalism" beneficial to all (page 345). He lauds the "achievements" of the bureaucratic state and the highly organized special-interest groups that character-

ize advanced societies today. Indeed, these institutional developments, denominated as corporate pluralism, deserve more credit in his moral calculus than the outcome of World War II for the stabilization of Europe.

How well does Professor Maier's model fit the facts? For one thing, the diversity of capitalist enterprise robs the notion of self-conscious bourgeois defense over fifty years of any precise meaning. Moreover, after the oil crises of the 1970s, economic equilibrium does not seem so permanent after all. Distributive contests do not lie in the past: they remain at the heart of politics everywhere. To be sure, international stability has endured in Europe. But that accomplishment derives from the final solution of the German problem through the three-fold division of Bismarck's Reich, from the democratization of the Federal Republic (and Japan) owing largely to American occupation, and from the energetic defense of free societies by the North Atlantic Treaty Organization. Moreover, World War II not only defined new boundaries and altered mentalities; it also hastened the metamorphosis of social institutions—not least by channeling state resources into science and quickening the pace of technological change. How could we conceive of the new Europe without superhighways, air networks, and high-speed trains, telephones and television for all, the replacement of the peasantry by agribusiness, the extraordinary mobility of labor and capital in the Common Market, and the standardization of taste that makes feasible the marketing strategy of the multinational corporation? It seems remarkable that diplomacy and technology play such subordinate roles in Maier's explanatory scheme.

Still more puzzling is Professor Maier's intimation that public spending and income redistribution provided a basis for balanced growth and an end to ideology. Surely this doctrine constitutes a curious survival of 1960s-style Keynesian hubris. A generation of stupendous economic advance after 1945 actually sprang from a fortunate confluence of circumstances. These included a new business mentality, a backlog of readily applicable technology, and an abundant supply of labor in the late 1940s, sound monetary policies and declining unit labor costs in the 1950s, and relatively low energy and raw-material prices in the 1960s. The experience of the last decade suggests that, in the absence of favorable exogenous influences, the "politics of productivity" (page 345) requires state complicity in the inflation of money and credit in order to sustain high aggregate demand.

SPECIALISTS MAY, HOWEVER, BE MORE TROUBLED by what Professor Maier has to say specifically about the 1920s and 1940s than by shortcomings in his conceptual scheme. Satisfactory clarification would require an exegesis as long as his essay. I can touch here on only two topics: the purported "fundamental challenge" to "managerial control" in the early 1920s (pages 334–35) and the motive forces for European recovery after 1945.

Professor Maier tends to exaggerate the threat posed by the West European Left in 1918–21. In what major Western country did the working classes fail to fight faithfully in World War I for what he calls an "alien upper-class cause" (page

331)? In France the authorities attributed the mutinies of 1917 exclusively to poor food and insufficient leave; they pretended to view the postwar strikes as a serious menace only when it suited their electoral purposes to do so.¹ In Great Britain the lucubrations of the Clydeside dissidents scarcely registered at cabinet level.² In Belgium the population was caught up in a nationalist frenzy.³ In western Germany, a majority of the citizenry welcomed Allied troops in 1919 as a surety against communist-fomented disorder.⁴ The factory-council movement flourished mainly in those parts of Germany and Italy where state authority had broken down. Factory commissions could scarcely run a complex industrial society; in practice, they fell apart upon the reconstitution of effective government.⁵ It strains credence to think that businessmen worried about the "legitimacy" of ownership in the 1920s or that they took seriously socialist criticisms of managerial responsibility.⁶ Businessmen before the Depression felt quite secure about their social roles. Where is the evidence that the professionalization of management, the development of multidivisional structure, or the rationalization of industry in advanced countries constituted a response to a perceived threat from the Left?⁷

Professor Maier believes that European reconstruction after World War II owed much to the cooperation of farsighted working-class leaders who helped moderate wage demands. Direct American aid, he asserts, played a modest role in the recovery. Rather, a new generation of Keynesian-influenced administrators resisted American pressure for deflation and convertibility and generated growth by taking international deficits in stride. All of these claims strike me as

¹ See the reports on the control of postal correspondence in Service Historique de l'Armée, Vincennes, 6 N 146-47; and the analyses by the prefects in Rapports sur l'esprit public et les élections, Archives Nationales, Paris, F¹ IIIC.

² There is no more than passing mention of a domestic Bolshevik threat in the records of cabinet meetings (Public Record Office, London, CAB 23) or in the papers of the principal members of the War and Coalition Cabinets, including David Lloyd George and Andrew Bonar Law (House of Lords Record Office, London), Austen Chamberlain (Birmingham University Library, Birmingham), and Earl Curzon (India Office Library, London). Even New Scotland Yard pronounced home-grown Bolshevism of little importance; see "The Progress of Bolshevism in Europe," January 28, 1919, copy in the Edward M. House Papers, box 207, Yale University Library.

³ See, for example, Pierre Nothomb, "Dix ans de politique nationale," Pierre Nothomb Papers, carton 82, Bibliothèque de l'Université de Louvain-la-Neuve, Louvain-la-Neuve.

⁴ See the striking reports by the commander of the American Occupation Forces in the Henry T. Allen Papers, cartons 11-15, Library of Congress, Washington.

⁵ This is apparent even from works on which Maier relies: Eberhard Kolb, *Arbeiterräte in der deutschen Innenpolitik, 1918-1919* (Düsseldorf, 1962); Peter von Oertzen, *Betriebsräte in der Novemberrevolution* (Düsseldorf, 1963); and Mario Abrati, *La Lotta sindacale nella industrializzazione in Italia, 1906-1926* (Turin, 1967). For Maier's detailed argument, see his *Recasting Bourgeois Europe* (Princeton, 1975), 136-94, 559-72; for a corrective that emphasizes the limited appeal of the council movement to German trade unionists, see David W. Morgan's *The Socialist Left and the German Revolution* (Ithaca, N.Y., 1975).

⁶ This is not to deny the importance of the subject for an intellectual biography of Antonio Gramsci or of his influence on such post-World War II socialists as Rodolfo Morandi.

⁷ The evidence is particularly well developed for the United States—the leading industrial power. See James W. Prothro, *The Dollar Decade* (Baton Rouge, 1954); and Alfred D. Chandler, Jr., *Strategy and Structure: Chapters in the History of the American Industrial Enterprise* (Cambridge, Mass., 1962), and *The Visible Hand: The Managerial Revolution in American Business* (Cambridge, Mass., 1977), 455-83. My judgment on the self-confidence of French and German business derives from reading the files of Saint-Gobain-Pont-à-Mousson, La Châtre; Gutehoffnungshütte, Oberhausen; Verein deutscher Eisen- und Stahlindustrieller, Bundesarchiv, Koblenz; and other industrial archives. Only in Italy, the least advanced of the industrial nations, did an anxious business class react forcefully to fundamental challenge—by working out a mutually satisfactory, if wary, accommodation with Mussolini's regime. See Roland Sarti, *Fascism and the Industrial Leadership in Italy, 1919-1940* (Berkeley and Los Angeles, 1971).

iconoclastic at best. No doubt the Communist leadership did try to control the rank and file for two years after the war, but only for tactical reasons—as the reversal in 1947 under Cominform orders makes clear.⁸ Furthermore, as long as Communists remained part of the French and Italian governing coalitions, they constituted an insurmountable psychological obstacle to investment. Only upon their exclusion from power could secure reconstruction begin. Thus, concrete American encouragement for the Mouvement Républicain Populaire and the moderate socialists in France, and for the Christian Democrats in Italy, proved vital in 1947–48. Similarly, the Bizonal administration barred the route to disruptive radical challenge in Germany. Moreover, American economic aid provided the crucial margin that eliminated bottlenecks in transport and raw-materials supply, coaxed hoarded goods and gold on to the market, and paved the way for the all-important conquest of inflation. The simple ratio of this assistance to gross domestic capital formation (even if one accepts Maier's figures) rather understates the American contribution.⁹

It is perilous to generalize about the economic prescriptions that American policymakers urged on Europe because New Deal “planners,” Hullian free-traders, and the hard-nosed realists in the Department of State's Office of European Affairs fought a protracted bureaucratic battle over their respective views. Yet it seems misleading to say even that the Treasury “insisted stubbornly on convertibility” (page 344). No doubt the Treasury required that Great Britain stop excluding American trade from the sterling area as the price of its 1945 loan, but toward the Continental nations it pursued a flexible and pragmatic policy.¹⁰ After 1948, contrary to what Professor Maier suggests, Marshall Plan administrators urged deployment of the counterpart funds to boost consumer demand, even at the risk of inflation. The Europeans resisted. In Germany, Ludwig Erhard conjured up an “economic miracle” after introducing a new currency. He did so by restricting fiduciary circulation and credit, sheltering business from taxation, fostering export growth, acquiring foreign reserves to build confidence, and facing down union demands.¹¹ In Italy as well, proponents

⁸ In so far as one can pierce the veil of secrecy, it appears that the French Communists hoped a coming Depression would provide the objective conditions for revolution. In Italy, Togliatti recognized the weakness of the working class and the need for caution to build an alliance with the southern peasantry and the displaced segments of the middle class. See Alfred Rieber, *Stalin and the French Communist Party, 1941–1947* (New York, 1962). On Italy, see John Harper, “The United States and the Italian Economy, 1945–1948” (Ph.D. dissertation, Johns Hopkins University, 1981). Harper has summarized a vast literature, and I draw heavily on his work below.

⁹ In note 36, page 342, above, Maier accounts for only \$9.4 billion in aid. But the Economic Cooperation Administration actually disbursed \$13.5 billion from 1948 through 1952, on top of \$10 billion transferred earlier to Western Europe by U.S. government and international agencies and private relief organizations. See the calculations in Richard Mayne, *The Recovery of Europe, 1945–1973* (Garden City, N.Y., 1973), 144–45.

¹⁰ For an explanation of sterling convertibility and the sterling-balances issue, see Richard Gardner, *Sterling-Dollar Diplomacy in Current Perspective* (New York, 1980), 188–268. Also see Alfred E. Eckes, Jr., *A Search for Solvency: Bretton Woods and the International Monetary System, 1941–1971* (Austin, 1975), 165–236.

¹¹ From June 1948 through December 1953, the gross national product rose roughly 304 percent, industrial production 221 percent, and exports 351 percent. But the money supply grew only 106 percent. Real wages (from 1950) increased on average by no more than 11 percent per year, while mean unemployment remained at 8.8 percent—almost twice the figure before currency reform. I have extrapolated these figures from Gustav Stolper *et al.*, *The German Economy, 1870 to the Present*, trans. Toni Stolper (New York, 1967), 247–51, 290; and Ludwig Erhard, *Deutsche Wirtschaftspolitik: Der Weg der sozialen Marktwirtschaft* (Düsseldorf, 1962), 622–23. For an expression of Erhard's free-market faith, also see his *Wohlstand für Alle* (Düsseldorf, 1957).

of liberal economics (President Luigi Einaudi and his protégés, Giuseppe Pella of the Budget Ministry and Donato Menichella of the Bank of Italy) ignored American promptings for social reform and Keynesian spending programs. They brilliantly conceived and implemented an orthodox program of stabilization through deflation, sterilizing the counterpart funds.¹² In France the course of events proceeded less smoothly, but old-fashioned monetary stabilization imposed by conservative Antoine Pinay in 1952 almost surely laid the basis for the subsequent business boom.¹³ So much for the politics of productivity!

PROFESSOR CHARLES MAIER EVOKES a historical universe in which idealism still holds sway. History has "tasks," centuries receive "sociopolitical assignments," and governments are "called upon" by unnamed powers to maintain economic growth and high employment at specified levels of income. Not all of the foxes of the historical profession, committed to a documentary orientation, will find it easy to follow Maier as he ventures on this terrain. Nevertheless, his pioneering work underscores the need for synthesis in contemporary European history. It is for those who believe in the merits of the inductive method to take up the challenge.

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¹² Harper, "The United States and the Italian Economy, 1945-1948," chap. 9. Also see George H. Hildebrand, *Growth and Structure in the Economy of Modern Italy* (Cambridge, Mass., 1965).

¹³ For a political account, see Georgette Elgey, *Histoire de la IV^e République*, 2 (Paris, 1969): 47-82; and, for a technical analysis, see Sylviane Guillaumont-Jeanneney, *Politique monétaire et croissance économique en France, 1950-1966* (Paris, 1969); and Jean-Jacques Carré et al., *La Croissance française: Un Essai d'analyse économique causale de l'après-guerre* (Paris, 1972), 437-92.



CHARLES MAIER'S THESIS IS THAT the postwar periods after World Wars I and II form parts of the same effort of stabilization not of procedures of democracy but of overlapping hierarchies of power, wealth, and status that we can loosely term capitalist. In another passage he calls these hierarchies "elites superintending Western society" (pages 333-34). I would quarrel with this position on two scores: (1) the concept of power elites that make "efforts" over two continents and half a century, and (2) the notion that the two postwar periods form part of the same effort.

THE FIRST ISSUE IS QUICKLY DISMISSED. Both the extreme Left and the extreme Right embrace conspiracy theories in which the other polar group makes deci-

sions and carries out efforts. Ex ante, this is nonsense in social science; ex post, social determinism may make it look as if such anthropomorphism were justified, though it is not. And the notion of a single, even overlapping, power elite in a dozen countries over half a century is also unacceptable. For an extreme example, look at John Porter's *The Vertical Mosaic: An Analysis of Social Class and Power in Canada* (1965). Porter is a disciple of C. Wright Mills and has argued that the six elites in Canada—economic, labor, political, mass media, intellectual, and religious (clerical)—form a unit. It happens, however, that his economic elite is from American multinational corporations, the political elite from Canada, and the bishops of the Anglican church from England. To find a decision-making apparatus and a converging effort there wielding power takes doing. To extend the elite to more countries and a longer period challenges belief.

More fundamental, I find it impossible to accept the thesis that the interwar period and the period after World War II are umbilically connected beyond mere calendar and time geography. Professor Maier tries to equate the interest of Benjamin Strong and the J. P. Morgan partners in European monetary conditions in the 1920s with the economic program of the United States as a whole after 1945. It is farfetched. On any reasonable showing, the United States was isolationist in the 1920s and deeply engaged at the end of World War II. He implies that American and European efforts at expanding trade, stabilizing commodity markets, and international monetary reform were comparable in the two periods. I leave it to the reader to make the contrast between the Brussels, Genoa, League-of-Nations-Gold-Delegation efforts on the one hand and the one-by-one stabilization of the Reichsmark, the overvaluation of the pound, the undervaluation of the French franc, and the like on the other, with the full-scale program of Bretton Woods, the Anglo-American Loan Agreement, the Marshall Plan, the European Payments Union, and so forth. Few today remember the Stevenson rubber plan of 1924—a foretaste of the OPEC destabilizing operations of the 1970s—or the failure of the World Economic Conference of 1927, which agreed on a program of lowering tariffs (although no country took action), or the League of Nations follow-up, which sought world-wide commodity agreements in cement and aluminum but aborted.¹ The comparison between the steel agreements of 1920 and the European Coal and Steel Community is specious. For one thing, the ECSC contained provisions opposing cartels. For another, Maier exaggerates its success as it broke down—Diebold to the contrary notwithstanding—and was swept under the rug of the Rome Treaty of 1957.

I suppose I should feel on shakier ground in objecting to Professor Maier's failure to dwell on the contrast between the German inflation of 1923 and the monetary reform of 1948. The one mention of the latter is coupled with the remark that both Germans and Americans wanted it postponed, citing a source

¹ See my "Commercial Policy between the Wars," in the *Cambridge Economic History of Europe*, 8 (forthcoming).

inaccessible to me (page 343). The Colm-Dodge-Goldsmith report was commissioned by the Department of State in the spring of 1946 and finished that summer. Delay in its implementation largely turned on the difficulties of printing the new money, which took six months, confused by the difficulty of choosing where to do it. The *Reichsdruckerei* was located in the Soviet zone of occupation, in Leipzig, and there was a widespread, and mistaken, belief in the United States that the losses suffered by the U.S. Army in occupation currency had been caused by the action of the U.S. Treasury in turning over the plates for printing occupation currency to the Soviet Union. When it finally came, moreover, monetary reform took place in stages, the monetary and debt conversion in June 1948 and the *Lastenausgleich*, a mortgage on all real property to equalize war losses, later, on September 2, 1948, when the German administration took power. The delay was the result of a veto of the *Lastenausgleich* by the secretary of the army, Kenneth Royal, on ideological grounds. He was less interested in stabilizing German society than he was in opposing capital levies everywhere, because they might be infectious. A number of recent historians—Gerald Feldman, Charles Maier, Stephen Schuker—seem to believe that the German inflation of 1923 was of no real destabilizing force—no worse, so to speak, than a bad cold—as opposed to a major force tearing at the social fabric of the country and paving the way, along with the unemployment of 1929–32, for the Nazis. I simply do not believe it. In my judgment, the monetary reform of 1948 was a great feat of social engineering, in sharp contrast with 1923.

One could go on: The General Strike in Britain in 1926 was hardly part of an effort of stabilization, although its defeat may have been; nor were Italian fascism and French right-wing efforts by many groups, large and small, which failed to converge. The international restraints on recovery in the 1920s, as Professor Maier calls them (war debts, reparations, skewed capital movements), might have been overcome had there been a consistent international effort at social stabilization. Instead, short-run, national interests dominated, and the fallacy of composition produced the Great Depression, as destabilizing an effort as one can imagine.

NOR CAN I ACCEPT the second half of Professor Maier's thesis. In his view, stabilization was achieved in two stages: in the first, dissent was repressed in the inter-war period. In the second, capitalism finished the job by delivering the goods. No country sought alternatives to the old-fashioned, liberal, market capitalism. In my judgment, to the contrary, various countries tried various alternatives and abandoned them when they failed. Nationalization of central banks, commercial banks in France, and various industries took place in France and Britain, with limited, if any, success. Britain's Labour Government started out to plan foreign trade through bilateral trade agreements, only to find that, although it was agreeable to insist on low prices for pre-Korean bargains, it was intolerable to be held to post-Korean contracts when prices fell after 1951. A

change of government provided a convenient moment to abandon planned foreign trade, but the Labour Party was already disenchanted.

Nor is French planning featured in what Professor Maier terms the steps leading to the maintenance of capitalist elitist domination. Planning was conceived by Monnet as an alternative to a "resolute market capitalism." Andrew Shonfield hailed it as *Modern Capitalism* (1965), but it is a far cry from the liberal solution. Maier dismisses planning after a brief mention, in which he connects it to Vichy and the 1930s (page 331). The origins, of course, go back at least to the Saint-Simonism of the early nineteenth century, and it is not surprising to learn that the Corps des Ponts et Chaussées was asked by Minister Roland in 1792 to prepare a plan for the postwar economic order, including, along with public works, plantations, mines and quarries, forges, and factories.² But French planning, like Keynesian demand management in Britain (and the United States), proved a failure. Like the ECSC, it was allowed to fade away rather than be publicly buried. Maier's surprise that other "collectivist alternatives" were not tried is surprising to me. The economic results of the Soviet system or of German or Italian fascism appealed to a few, perhaps, but the political costs were generally taken to be intolerable.

Professor Maier also seems in error when he implies that all European countries did more or less equally well after World War II. This is not so. The rates of growth in Britain and Belgium (plus the United States and Canada) were the lowest in the League standings for reasons on which economists differ, as they do on those for the successes of Germany, Japan, France, Italy, the Netherlands, and, belatedly, Norway. Space is lacking to recapitulate the various views beyond cryptic designations: Maddison, demand management; Andrew Shonfield, planning; Charles Kindleberger, unlimited supplies of labor. But a new view by Mancur Olson, Jr., runs diametrically counter to Maier's emphasis on "corporate pluralism" of "trade unions, ambitious state economic agencies, and bureaucratized pressure groups" (page 351). Olson has suggested that defeat or occupation purged these countries of collective interests—trade associations, cartels, trade unions—and required individuals to go back and work on their own. Collective interests in Britain and the United States gained from victory and grew to the point where their efforts to achieve collective gains interfered with each other and with the national interest.³ World War I discredited the German monarchy but virtually nothing else in that country—not the army, the Junkers, or industry. The Thyssens and the Helfferichs continued to wield power after Versailles as before 1914, as Weimar struggled ineffectually to govern. Germany was able to accept the equalization of war losses (*Lastenausgleich*) because there were no groups able and ready to force the burdens of the war on to others.

² Commission de Recherche et de Publication des Documents relatif à la vie économique de la Révolution, volume 9: *Fragments d'une enquête économique de Ministre Roland auprès des ingénieurs des Ponts et Chaussées* (Besançon, 1942), 2, 9.

³ Olson, *The Logic of Collective Action: Public Goods and the Theory Group* (Cambridge, Mass., 1965).

OF COURSE, ONE CAN FIND SIMILARITIES AND DIFFERENCES in postwar periods. War is a hothouse: it accelerates the growth of some tendencies and hastens the decline of those on the downward path. In a forthcoming paper, I compare the resumption of gold payments in Britain in 1819 and the revaluation of the pound in 1925 and find strong similarities.⁴ It is not unexpected that Professor Maier can find similarities as well as differences between the two postwar periods.

I find it bizarre, however, to conclude that both were part of the same piece. What is interesting, I submit, is how much 1945 learned from the mistakes of 1919, in a world where it is easy to draw the wrong lesson—for example, the Maginot Line—or not to learn at all—as, for example, 1925 did not learn from 1819. The United States, for one, did not try to withdraw from the European settlement—not for the reason that Charles Maier adduces, to keep Europe resolutely capitalist and market-oriented—and corrected the mistakes of war debts, reparations, wrong exchange rates, and niggardly postwar assistance. Europe's delivery of goods—a remarkable episode in the history of economic growth—was based less on lessons learned from the past than on accident and continued questing, trying new expedients when old paths seemed not to lead anywhere.

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⁴ Kindleberger, "British Financial Reconstruction, 1815–1822 and 1918–1925" (forthcoming).

Reply:

I HAVE ENJOYED AND LEARNED SO MUCH from Charles P. Kindleberger's books that I must give considerable weight to his objections. Nonetheless, I do think that he has reacted very sharply to some points that might seem less exceptional upon rereading. Stephen A. Schuker's critique rests more upon general methodological considerations and some differing economic premises; in response I shall try to point out where our views converge and where they are likely to remain subject to further debate.

BY REFERRING TO ELITES, I do not propose C. Wright Mills's concept of one interlocking power elite (against which Daniel Bell leveled some disabling objections two decades ago).¹ It seems too self-evident to belabor that some individuals and groups enjoy more influence over political decisions and economic arrangements than do others, often more numerous. To suggest that overlapping circles of civil servants, elected politicians, eminent businessmen, and sometimes intellectuals, including academics, have played continuing roles in shaping public outcomes hardly amounts to a conspiracy theory. Nor does observing that, although we elect by a democratic process important leaders with clear political responsibility, many of the men and women who manage our bureaucracies, institutions, and large-scale economic units are merely coopted.

I do not equate, moreover, the events of the 1920s with those of the 1940s and 1950s. As I stated in the essay, I deny identity but discern parallels. I do not think that the concepts of stability and of the value of broad economic intercourse held by the policymakers of the 1940s were so different from the ideas of the more internationalist businessmen and statesmen of the 1920s. Historical research of the past generation has suggested that American "isolation" is relative at best: useful, perhaps, to characterize a fear of security commitments that climaxed with the Neutrality Acts but less revealing for the economic involvements of the 1920s. And, for every contrast between the currency and economic developments of the 1920s and those after 1945, I find an instructive, not merely a captious, comparison. Of course, the scale of economic intervention was far more intense and successful after World War II. That was one of my major points. But many of the structural dilemmas (those created, for example, by the postwar creditor position of the United States) emerged at both junctures. For Professor Kindleberger, the change in degree between the blighted initiatives of the 1920s and the successes of the second era renders the com-

¹ Bell, "Is There a Ruling Class in America? The Power Elite Reconsidered," in his *The End of Ideology: On the Exhaustion of Political Ideas in the Fifties* (New York, 1960), 47-74.

parison invalid. For me, it suggests caution but does not remove the heuristic value.

To take two specific issues, consider the steel agreements and currency reform. The juxtaposing of the steel agreements of the 1920s and the later Coal-Steel Community is perforce incomplete, but it is not "specious." Granted, the earlier experience responded to saturated markets and encouraged quotas, whereas the ECSC emerged while output was still low. In both cases, however, Germans and French sought to integrate leading industrial sectors at the cost of some national industrial independence. In both cases, the French exploited a transitory superiority to negotiate agreements that were designed to compensate them for their longer-term vulnerability; in both cases, international agreement accompanied other supranational political linkages.

Concerning the German currency reforms, my arguments on the inflation of 1918–23, which are set out elsewhere, sought to emphasize that the views of inflation "destroying" the German middle classes or delivering them to Hitler were too simple and that the way in which inflations are ended can be as politically devastating as inflation itself.² The currency reform of 1948—I, too, believe—was engineered well, but it could have been instituted a year or so earlier; the costs of the delay were harsh in real, as well as in monetary, terms. Indeed, when Professor Kindleberger admits that revaluation was delayed on "ideological" grounds, I think he really buttresses my arguments. I would note, too, that the *Lastenausgleich*, or compensatory levy, instituted in stages in 1948 and after 1952 was less thoroughgoing than the one envisaged in the Colm-Dodge-Goldsmith plan. The original proposal to couple economic redistribution with technical monetary reform was shelved. Moreover, the C-D-G plan foresaw a redistribution of assets partly to compensate for war losses but partly to equalize the starting position of those who had preserved only paper claims (bank accounts, bonds) with those who had houses or business plants (largely freed of incumbent debt). The later *Lastenausgleich* abandoned this broader scope; again, an instructive comparison can be made with the limited revaluation of debts and claims in 1924–25. But in the new conditions of economic growth after 1948, the limits made less difference.³

When Professor Kindleberger objects to the "second half" of my thesis (the stages of stability), I must protest on several grounds. I do not claim that dissent was merely repressed in the first postwar period; the argument stresses ideological relegitimatization. Kindleberger writes that various countries tried various

² See Charles S. Maier, "Die deutsche Inflation als Verteilungskonflikt: Soziale Ursachen und Auswirkungen im internationalen Vergleich," in Otto Büsch and Gerald Feldman, eds., *Historische Prozesse der deutschen Inflation, 1914 bis 1922* (Berlin, 1978), 329–42, and *Recasting Bourgeois Europe: Stabilization in France, Germany, and Italy in the Decade after World War I* (Princeton, 1975), 358–64, 483–94. (Professor Schuker, by the way, dissents from my emphasis and would presumably feel uncomfortable with Professor Kindleberger's joining us all.) Obviously, the hyperinflation was worse than a common cold; but German democracy was afflicted with several potentially lethal ailments.

³ For the currency reform and *Lastenausgleich*, see, among other sources, Hans Möller, "Die westdeutsche Währungsreform von 1948," in *Währung und Wirtschaft in Deutschland, 1876–1975* (Frankfurt a/M, 1976), 433–83; and G. Weissner, "Die Gesetzgebung über den Lastenausgleich: Bemerkungen zu ihrer Geschichte und kritische Analyse ihrer Grundgedanken," *Finanzarchiv*, new ser., 16 (1955): 62–80.

economic alternatives after 1945, only to abandon them when they failed. Is this so different from my view that "throughout the first three postwar years, in fact, there was less decisive purpose than confused experimentation and uncertain initiatives" (page 343)? Whether Jean Monnet's planning was really an alternative to capitalism or a variant of it remains the subject of debate. The interpenetration of political and economic decision makers does not allow easy typology for France any more than it does for most other industrial societies.

Finally, Mancur Olson, Jr.'s, argument, which emerges most clearly in his recent presentations,⁴ that the thicket of interest groups in the United States and Britain has impeded economic growth, whereas the shattering of their equivalents in Germany and Japan unleashed new energy, seems too simple to me. I submit, contrary to my understanding of Olson's work, that not the density of interest groups but the degree of cohesion or lack thereof separates the economic leaders from the laggards. Anglo-American liberalism has been updated into an adversarial and bumbling pluralism; in the growth leaders, interest groups have been certainly dense (and sprang back quickly) but have built upon traditions of premodern social discipline.

Of course, this is an argument for continuity, which was the burden of my essay. Yes, 1945 learned from the mistakes of 1919: indeed, Charles Kindleberger was one of those to apply the lessons with enthusiasm and good sense when he was at the Department of State. This hardly means he could not have been contributing to an effort at stabilization (and renewal), the terms of which had been outlined earlier. I did not say that the United States intervened to save capitalism and the market. We stayed in Western Europe because we did not like the Soviet presence in Eastern Europe. But to state that is not to deny that the upshot of events was a version of market liberalism relatively close to our preferences.

DESPITE STEPHEN SCHUKER'S EMPHASIS upon our Christian Democratic interlocutors, I still feel that farsighted American policymakers perceived that buttressing social democracy was necessary to save both liberty and markets (page 346). Washington had to wager upon the forces of the center, hence on De Gaspari and even, exasperatedly, on Bidault; but, until 1950 or so, officials persisted in viewing social democracy as an ideological fulcrum.

Many of Professor Schuker's critiques do not significantly diverge from arguments in my essay. It is true, as he states, that distributive conflicts hardly lie in the past alone. As I intimate in my conclusion (and state on pages 340-41), however, the very recent period (say, since 1973 or so) concluded a quarter-century marked by their astonishingly low level. To be sure, the broad forces of technology (as well as of mass education, leisure, and changing family and sexual mores) are absent from my account. As I state explicitly, I do not wish to deny

⁴ Olson, "The Political Economy of Comparative Growth Rates," in James Gapinski and Charles Rockwood, eds., *Essays in Post-Keynesian Inflation* (Cambridge, Mass., 1979), 137-59.

social transformation, only to emphasize the political continuity even as society altered.

The “fortunate confluence of circumstances” (page 355) that Professor Schuker finds responsible for post-1945 economic growth is not so different from the causes to which I refer (pages 343–44). In general, I am less convinced than my critic that monetary stabilization is the motor of growth. (Einaudi’s brilliance may have helped perpetuate southern Italian dualism. Erhard inherited a currency reform, a Social Democratic party at an impasse, and the West’s crying need for German resources.) Ultimately, it may require growth to make monetary stabilization “stick.” Both those who stress monetary factors and those who argue for structural or political ones can provide coherent models. What is challenging for the historian is that the differing approaches to questions of economic policy today must be present in any assessment of past issues. This holds for the dilemmas of the latter 1920s, for policy in the Depression (where monetarist and Keynesian accounts clearly compete), for the issues of 1945 and after. No historical issue, I suspect, can be resolved with any degree of certainty greater than is granted to the economic prescriptions for analogous contemporary problems.

There is room to differ over the role of Marshall Plan aid. I believe that it was critical first in demonstrating an American political commitment and second in overcoming balance-of-payment constraints—but only to a subsidiary degree (and this was the point of my calculations) in rebuilding Europe in the crude sense often invoked. In this regard, the discrepancy between the total aid to each country in Professor Schuker’s note 9 (page 357) and my own in note 36 (page 342) is readily explainable. I deliberately excluded pre-Interim Aid (pre-eminently UNRRA relief) and post-1951 Mutual Security assistance, largely slated for rearmament. I also focused only on the four major recipients. I do not see where I deny that ECA officials urged deployment of counterpart funds to boost demand; indeed, I identify them as the Keynesian bulwark in the making of foreign economic policy, and I think the sources will confirm my reading of the contrast with the Treasury. Finally, my own comparison of the dilemmas of the late 1920s with contemporary difficulties (see page 340) hardly corresponds to any Keynesian hubris.

I find more to the point Professor Schuker’s criticism that I exaggerate the threat of the Left and the managerial response of the conservatives after World War I. My argument here, however, relies (as I note) not on actual power but on alternative ideological visions. For the legitimacy of capitalism, the battle between ideologies and utopias of the productive order did remain critical. What is more, leading defenders of capitalism—Olivetti, Vögler, Rathenau, Mercier, Mond—recognized this. The point is not that fear of Bolshevism directly motivated modernization of plant, improvements in technology (such as cogeneration), mergers and coordinated planning (such as electricity networks), and Tayloristic efforts at labor rationalization. Rather, these changes were prodded by, and in turn encouraged, a confidence in the mission of managerial capitalism after it had overcome a sharp challenge.

Professor Schuker’s skeptical, but thoughtful, general assessment may illus-

trate, as he suggests, the natural differences that can result from more or less inductive approaches. Nonetheless, I would be reluctant to accept the designation of a deductive historian. The length allotted to an essay does not permit retracing all of the trails through lowlands and woods that have led to the ridges and vistas. This does not mean that the historian has not traversed them. Schuker suggests that my use of the term capitalism is vague enough to mean bourgeois democracy. Fair enough for the period since 1945—if the adjective remains in view. More generally, I think, he speaks for the many who distrust using ideal types for historical summation. The *American Historical Review* has recently featured learned objections to the concepts of feudalism and fascism.⁵ Clearly, it is easy to cite cases that depart from any typology, which by its nature must simplify. Yet employing political constructs need not require burdening them emotionally in advance, nor need it involve renouncing the complementary search for variation. But how can the historian dispense with generalization? Forests as well as trees undergo phases of growth and destruction.

I OFFER THIS PIECE NOT TO CLAIM that it represents *the* correct interpretation but to present one way to think about the links between the crises and the recovery of twentieth-century European society. When I presented the paper at the American Historical Association meetings of December 1978, Professor J. H. Hexter admitted sympathetically that it had been stimulating but wondered if, after all, the real reason for stability following World War II did not derive from the simple fact that people were tired of despotism. The observation was sobering in its implication that my account might be overstructured. Let me respond two years later that people rarely want despotism, or war, or idle factories, or worthless money. But they sometimes get them. Here I have tried to outline the circumstances that let at least some of them translate yearnings for normalcy into the liberal capitalist efflorescence that followed the great wars of our century.

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⁵ Elizabeth A. R. Brown, "The Tyranny of a Construct: Feudalism and Historians of Medieval Europe," *AHR*, 79 (1974): 1063–88; and Gilbert Allardyce, "What Fascism Is Not: Thoughts on the Deflation of a Concept," *ibid.*, 84 (1979): 367–88.

Reviews of Books

GENERAL

FERNAND BRAUDEL. *Civilisation matérielle, économie et capitalisme, XV^e-XVIII^e siècle*. Volume 1, *Les structures du quotidien: Le possible et l'impossible*; volume 2, *Les jeux de l'échange*; volume 3, *Le Temps du monde*. Paris: Armand Colin. 1979. Pp. 543; 599; 606.

As guides to unfamiliar terrain, some historians dismay us by stumbling over their footnotes, losing themselves in blind alleys, and confusing the Parthenon with the Pantheon, until we long to grab the guidebook and wander on our own. The great guides, however, come from two different corps: mappers and musers. The mappers thrust a sketch of the terrain at us and start us marching along the main streets, ticking off the sites. It works: we see the Roman grid underlying Turin and the vast plan of Leningrad as if we had conceived them ourselves. The musers, in contrast, begin meandering with us, pointing out fascinating corners, turning abruptly to make connections we had never imagined. They leave us uncertain about the grand design, yet delighted by fresh perceptions.

Fernand Braudel sometimes talks like a mapper, but he is really one of the great musers. Two decades ago, his rambling survey of the sixteenth-century Mediterranean displayed an extraordinary sense of the interdependence among structures and changes that seemed remote from one another or even antithetical—for instance, the rise and fall of upland banditry as a function of fluctuations in lowland state power. Now he conveys that same sense at a scale that dwarfs the Mediterranean and the sixteenth century: his subject has become the experience of the entire world from the fifteenth through the eighteenth centuries. Even those four centuries do not contain him, as he moves backward to the Roman empire and forward to the 1970s. In three big volumes, Braudel attempts no less than a general account of the processes by which the capitalist world of the nineteenth and twentieth centuries took shape.

Braudel's account lacks the schematism of an H. G. Wells or a V. Gordon Childe. Complexities, nuances, contradictions, and doubts fill every chapter. The marvelous, abundant illustrations—plates, graphs, maps, diagrams, and tables by the hundreds occupy about a fifth of the text—nearly always lend new insights yet rarely fall neatly into a developing argument. Braudel's very prose bristles with complications; parentheses, dashes, colons, semicolons, and comma after comma mark off allusions, asides, qualifications, and repetitions. Not that the prose is dreary or obscure. On the contrary: Braudel writes with the verve of a restless lecturer who cannot stop recasting as he speaks.

He speaks at length. The main texts of the three volumes total to more than 800,000 words. The notes (inconveniently stored at the backs of the books and studded with mentions of *op. cit.* that lead back to distant references or to none at all) occupy another ninety dense pages. Many readers will feast on the references. Braudel has read widely and well in the Romance languages plus German and English. He gives us the benefit of that reading. What is more, the bibliography continues, varied and abundant, right up to items published (or circulating yet unpublished) in 1979. All things considered, few reflective readers of *Civilisation matérielle* will be able to sustain a pace as fast as 400 words per minute. The three volumes will therefore require at least thirty-five hours of brow-furrowing attention plus the time to dawdle over the illustrations and track down the notes.

To be sure, many of Braudel's readers will have a head start. An earlier version of volume 1 appeared in 1967. Braudel then presented it as the first (material-culture) half of a two-volume work called *Civilisation matérielle et capitalisme*. The new first volume follows the same general plan as the old but includes many editorial retouches, some sections that differ significantly in title and content from their predecessors, a set of notes (entirely absent from the earlier edition), references to many works published after 1967, and almost twice as many illustrations as before. The smaller alterations record the normal

outcome of another dozen years' reading, reflection, and response to criticism. The shift from a diptych to a triptych, however, represents more than a clever adaptation to an overgrown second half; the new organization gives more importance and autonomy to the short- and medium-run dynamics of economic activity than did Braudel's earlier statements on the subject. It also expresses a greater skepticism with respect to technological determinism and evolutionary processes. In the first edition (p. 329), for example, Braudel replied "yes and no" to the question, "Do techniques have their own history?"; in the new (vol. 1, p. 379), "the reply will surely be negative." Again, where he once considered credit to be a "luxury" that even the countries of Eastern Europe, with their "natural economies," lacked before the eighteenth century ([1967] p. 368), by 1979 (vol. 1, pp. 419–20) he saw some version of money and credit everywhere and only maintained that the range of monetary techniques expanded with economic development. So even faithful readers of the first edition will have something new to learn from the second.

As crystallized in titles and subtitles, the topic's three divisions now run: (1) material culture and the structure of everyday life, (2) economy and the workings of exchange, and (3) capitalism and world time. The breakdown does distinguish the emphases of the three volumes, but it does not reflect a causal hierarchy or a tight analytic model. In the first part, Braudel seeks to describe how the techniques of production, distribution, and consumption varied throughout the world—especially the Western world—over the four centuries after 1400 and to show how those techniques shaped everyday experience. In the second, he proceeds from a survey of the techniques by which people in different parts of that world exchanged goods to a discussion of various types and scales of markets. He then tries to identify the peculiarities of capitalism as activity and organization before examining its articulation with social hierarchies, state structures, and broad forms of civilization. The third part begins with a delineation of world-economies, as the fundamental units of analysis and continues with a roughly chronological portrayal of the successive world-economies that prevailed in Europe and elsewhere in the world. The survey is complicated by the simultaneous efforts to specify the changing places of smaller areas and individual cities within those world-economies, to trace the interactions among world-economies, and—as if that were not already enough—to explain how and why Europe finally became the world's master and its prime locus of large-scale industrialization.

Early in volume 2 Braudel calls explaining how the gap between Europe and the rest of the world appeared so late and yet became so decisive "the es-

sential problem in the history of the modern world" (vol. 2, p. 111). By page 481 of volume 3, nevertheless, he offers an indirect admission of theoretical defeat: "the Industrial Revolution which overturned England, and then the whole world, was never, at any point in its path, a precisely delimited subject, a given bundle of problems, in a particular place at a certain time." All the previous history recounted in this vast review, Braudel tells us, somehow converged on that outcome; the only way to analyze industrial growth is to break it into its many elements, to take up those elements one by one, and to trace their multiple connections. That Braudel's earlier analyses forecast just such an intellectual strategy, and that Braudel follows the strategy with subtle brilliance, do not eliminate a certain disappointment that our muser has not managed to transform himself into a mapper.

At the start of the third volume, it looks as though Braudel will try to perform his miracle by relying on Immanuel Wallerstein's model of the European world-system, especially its distinction of core, semiperiphery, and periphery. But Braudel eventually opts for a more relaxed identification of the world's economically independent regions, leans against Wallerstein's claim that the European capitalist world-economy was the first one not to consolidate into a political empire, and maps out multiple European world-economies well before the supposedly critical unification of the sixteenth century. He follows Wallerstein especially in building his account around the successive hegemonies of capitalist metropolises: Venice, Genoa, Antwerp, Amsterdam, London, New York. It is a grand story, elegantly told—and nothing like a definitive solution to the "essential problem."

Should we have expected anything else from a man of Braudel's intellectual temper? He approaches a problem by enumerating its elements, savoring its ironies, contradictions, and complexities, confronting the various theories scholars have proposed, and giving each theory its historical due. The sum of all theories, alas, is no theory. We end our long promenade delighted with all we have seen, grateful for our guide's wisdom and perspicacity, inspired to revisit some of the hidden junctions he has revealed, but no more than dimly aware of the master plan.

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DAVID E. STANNARD. *Shrinking History: On Freud and the Failure of Psychohistory*. New York: Oxford University Press. 1980. Pp. xx, 187. \$12.95.

It is a baffling fact that polemics against psychohistory draw more attention than works of psycho-

history. The latest notorious case in point, David E. Stannard's *Shrinking History*, has the peculiarity of being aimed point-blank, not at psychohistory itself, but at Freudianism. Stannard's reasoning here is that psychohistory is simply applied Freudianism and that the ills of Freudianism greatly exceed the pitfalls of applying it. True, in his first chapter Stannard knocks away at Freud's sloppy work on Leonardo da Vinci as the supposed prototype of psychohistorical scholarship, but that strained supposition looks for all the world like a mere debater's trick. At any rate, the other main counts of Stannard's indictment are on target against psychoanalysis: its therapeutic record is poor; it heavily overweights childhood; its logic is often self-confirming; its constructs do not pass experimental tests; they are culture-bound anyhow. These charges of Stannard's are all well attested. To my mind they are fairly foolproof, short of the last, overstretched one: the unconscious is hardly a culture-bound concept the way the Oedipus complex is. And they do add up against "history-as-applied-psychoanalysis."

The catch is that this would-be synonym of Stannard's for psychohistory is more of an antonym. Stannard's image of the psychohistorian is of a fact-sloppy biographer inferring childhood experiences from adult behavior to explain that selfsame adult behavior—drawing vicious circles in thin air above footnoted references to dubious clinical texts. Such malpractice does abound in the psychohistorical ranks: I, for one, have inveighed against it over the years. But it is not inherent in psychohistory. Ever more psychohistorians are working directly from full and straight facts with ever less regard for clinical theory. This should come as good news to Stannard in his relative ignorance of the field. For, Freudianism aside, historians out for explanations cannot escape psychohistory. History is what people did, singly or collectively. Why people did what they did means motives both conscious and, like it or not, unconscious.

This last point brings me to the big trouble with Stannard's critique of Freud, which is that it throws out the Freudian baby with the wash—the unconscious along with the Oedipus complex. Whoever once penetrates even a single dream of his own knows that the unconscious exists, experimental controls be damned. More, whoever once penetrates even a single dream of someone else's knows that empathy works. Stannard holds that empathy could never work across historical-cultural gaps because of the cognitive distances involved. But why can a psychohistorical researcher not grasp the different sense of time or love or truth that belongs to his subject? Correct. Stannard's brief against psychohistory for overkill and it comes to an unintentional case for psychohistory as a discipline in its

own right, independent of the Freudianism from which it derived.

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RICHARD TUCK. *Natural Rights Theories: Their Origin and Development*. New York: Cambridge University Press. 1979. Pp. viii, 185. \$24.50.

The scope of this historical monograph is limited to an analysis of jurisprudential and theoretical treatises concerning natural rights in the period between 1350 and 1670. The usual distinction between ancient and medieval "natural law" theories and modern "natural rights" theories is disregarded, and *ius* is translated as "right" throughout. The related distinctions, however, between "passive vs. active rights" and between "objective vs. subjective rights" permeate the monograph, and this leads to the generally accepted conclusion that ancient Roman law lacked true natural rights theory.

One senses the author's sympathy for the Utilitarian argument (which he traces to Pufendorf) that statements about individuals' passive rights are statements about others' duties. Richard Tuck's prerequisite for a true natural rights theory appears to be its defense of "active rights," that is, "to have the right to do something oneself" (p. 6), a concept integrated with concepts of property and liberty.

The philosophical underpinnings of Tuck's approach to natural rights theory provide the logical basis for his chronological scope. In an innovative interpretation, he traces the first rights theory to the 1402 work of Jean Gerson. The "Gersonian rights theory" emerged from the context of the medieval juristic study of Roman law. Aquinas appears only along with Azo and Ockham as predecessors to Gerson. Drawing further historical attention to the Universities of Tübingen and Paris, centers of Gersonian nominalism in the fifteenth century, Tuck suggests a first period of natural rights theory florescence, 1350–1450, to parallel the more widely accepted period of 1590–1670.

In line with a tendency in history of philosophy and history of science to find medieval origins of seventeenth-century thought, Tuck views the Renaissance as a backwater period in which natural rights theory was assaulted by Renaissance jurisprudence, Luther's theology, and Francisco de Vitoria's new scholasticism. This approach to the Renaissance would be modified by an interpreter who would give more significance to the abundant Renaissance natural law theory as background to seventeenth-century natural law and rights theories.

The major natural rights theorists of the classical period come under careful scrutiny and are newly

evaluated. In Tuck's interpretation, most natural right theorists were more authoritarian than liberal; thus, Hobbes appears as a representative, not atypical, figure in his endorsement of slavery and the absolutist state. John Selden and his followers in the English political scene of 1640–80 appear as the most important context for explicating Hobbes's thought; however, Tuck also gives full attention to the potential radical implications of natural rights theory as it was expounded during the English Civil War by Henry Parker, Richard Overton, and Anthony Ascham. Hugo Grotius receives full analysis as the founder of both the authoritarian and radical implications of the natural rights movement; John Locke is viewed rather briefly in the context of "the recovery and repudiation of Grotius."

The book makes a valuable contribution to historical re-evaluation of one of the most important, yet one of the most abstract, concepts in Western thought. The major defect is the unrelenting textual analysis. Tuck might have enlarged his audience by spicing up his prose with concrete examples from legal cases or by relating the conceptual development of natural rights theory to such changing institutions of early modern Europe as the slave trade and representative government.

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N. J. G. POUNDS. *An Historical Geography of Europe, 1500–1840*. New York: Cambridge University Press. 1979. Pp. xvi, 438. \$39.50.

The work under review constitutes the second and concluding volume of N. J. G. Pounds's *Historical Geography of Europe*. The first, published in 1973, presented an overview of the subject in five separate chronological cross sections between the years 450 B.C. and 1330 A.D. This one provides a closer scrutiny, organized "vertically" around five topics: population, cities, agriculture, manufacturing and mining, and trade, for the years between 1500 and 1840. Clearly the two volumes were intended to form a single work, but they can well, and probably will most often, be read as separate studies.

In his preface to this second volume Pounds announces that his purpose has been to introduce the "spatial dimension, so often lacking from history," into his account; and he adds that the results "must look remarkably like economic history." And so they do—like remarkably good economic history at that. Pounds has succeeded in presenting a lucid, comprehensive, and discriminating summary of the best monographic and secondary works dealing with this period; but whether intentionally or not, he has also implied that historical geography was

inherently economic in character. As a professional geographer, he is surely aware that virtually all kinds of history have their geographic dimension, with such major fields as political and military history yielding nothing in importance to the economic factor. Obviously this is not to suggest that it was a mistake for Pounds to concentrate on the economic or any other aspect of historical geography he might choose but merely that it would have been helpful if he had made his purpose more explicit.

A more serious, if less obvious, question is raised by the manner in which Pounds injects the geographic dimension into his canvass of the economic activity and development of early modern Europe. Although his systematic location of each phenomenon discussed is a solid contribution to his account, it hardly constitutes a geographic interpretation of economic history. His apparent reluctance to move beyond the "where"—the establishment of cartographic coordinates—to the "why"—the analysis of the human reaction to the physical situation that constitutes the essence of geography—is puzzling, to say the least. Although he seems to be as aware as any scholar now writing of the difficulty of moving bulky goods over land (before the mechanization of transport in the nineteenth century), he seems curiously reluctant to generalize about its significance for the social organization of Europe. Perhaps Pounds does not believe in injecting theoretical speculation into his history. If so, that would explain his apparent failure to draw on the work of such contemporaneous analysts of his subject as Richard Cantillon, François Quesnay, and J. von Thünen. To introduce the geographical dimension into history should not merely erect a stage for the human drama but also bring that setting into the story as a challenge to which the actors respond in the process of creating their particular society. If Pounds has not chosen to respond to that spatial challenge himself, he has nevertheless given us an excellent overview of the early modern economic history of Europe, set forth against the background of an attractive landscape.

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STANLEY G. PAYNE. *Fascism: Comparison and Definition*. Madison: University of Wisconsin Press. 1980. Pp. viii, 234. \$16.75.

This little work is described on the dustcover as "the best single, brief book on fascism available in any language." True enough. Stanley G. Payne presents an impressive review of reputed fascist movements, at once setting them apart from other authoritarian nationalist organizations and bring-

ing them together within a qualified generic category. Running throughout the volume, and valuable to readers at every level, is a careful critique of the major debates that divide scholars on this most unintelligible "ism" of them all. Payne precisely defines issues, cites the best literature in the major European languages, and offers with moderation and intelligence his own conclusions on the question. Indeed moderation is Payne's style—it goes with his subject. He writes, "My own conclusion, after two decades of examining various and manifold forms of fascistiana, is that . . . the common reduction of all putative fascisms to one single generic phenomenon of absolutely common identity is distortive and inaccurate, while a radically nominalist approach which insists that all . . . were inherently different . . . has the opposite defect of ignoring distinctive similarities" (p. 195). Offered is something in between, a loose, working description of traits common to all fascist movements. This "fascist minimum," Payne acknowledges, serves limited purposes: it reveals what is general to fascism, not what may be distinctive to any particular fascist organization.

Symbolic to fascism, it is said, is the marching column, men passing in file, faces blurring into a mass. Payne's résumé of political movements, great in number, worldwide in range, described in tight sentences and packed into few pages, sometimes creates the same impression. Complicating this is what he calls the "fascist vertigo" occurring after 1933, when all varieties of authoritarian nationalism, dizzy with Hitler's success, incorporated aspects of "fascistization." Some readers, therefore, will judge political identities to be less clear than Payne's definitions. Nazism, of course, is the hardest nut in every mix of fascist parties, and critics may find Payne's arguments here somewhat forced. Predictably, however, his summary of Spanish movements (his own specialty) is superb. Most important is the masterful quality of his larger project to sort out basic problems in the study of fascism as a whole. The phenomenon, he concludes, was grounded in a particular European cultural environment, was not reproduced in full in other areas, and is unlikely to reoccur in the future. Others have said this before, but no one has explained it so well.

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JÜRGEN HEIDEKING. *Areopag der Diplomaten: Die Pariser Botschafterkonferenz der alliierten Hauptmächte und die Probleme der europäischen Politik, 1920–1931*. (Historische Studien, number 436.) Husum: Matthiesen Verlag, 1979. Pp. 383. DM 94.

Jürgen Heideking's *Areopag der Diplomaten* is a welcome and timely addition to the ranks of first-rate

studies of European diplomatic history of the decade of the twenties. Heideking's work, submitted as a doctoral dissertation at the University of Tübingen, is based on the Hoover War Library microfilms of the documents of the ambassadorial conferences and on archival collections in the London Public Record Office and the foreign ministry in Bonn. The earlier works of Türckes, Pink, and others, which deal with the diplomacy of the victorious allied powers in their determination to maintain the status quo established by the victory of allied arms in 1918, did not have the advantage of all the archival and other sources that, with the opening of the ninety-nine microfilm sources in 1972 by the Hoover War Library, made possible what may very well become the definitive work on the conference.

Heideking's work is carefully put together. The author has, with great care, considered earlier works and primary sources in the construction of his diplomatic history. The organization leaves little to be desired. The writing is careful and succinct. Overall, Heideking has done a first-rate job, worthy of the best traditions of German scholarship.

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BRUCE ROBELLET KUNIHM. *The Origins of the Cold War in the Near East: Great Power Conflict and Diplomacy in Iran, Turkey, and Greece*. Princeton: Princeton University Press, 1980. Pp. xxiii, 485. Cloth \$27.50, paper \$10.50.

Bruce Robellet Kuniholm introduces this volume with the thesis that historians writing about the Cold War's origin have focused primarily on the conflict between the United States and the USSR in Eastern Europe, on the economic foundations of U.S. foreign policies, and on the impact of the atomic bomb, largely to the exclusion of adequate treatment of the power struggle between Russia and the United States along the "Northern Tier," that is, Greece-Turkey-Iran. He includes Afghanistan in this definition but excludes it from his treatment on the grounds that it did not figure seriously in the Cold War because at that time Afghanistan was still a "buffer state." Kuniholm uses the term Near East rather than the newer term Middle East (the latter invented by Alfred Thayer Mahan in 1902 to include the area of the Persian Gulf). He believes Near East is a more accurate designation since his history includes Greece.

The contents of this volume are divided into two parts: the sources of great power rivalry along the "Northern Tier" and the escalation of the Cold War.

Of much interest is the author's treatment of the first Iranian crisis of 1946, at which time the Soviet

Union did not pull back its occupation forces from northern Iran at the time of the agreed deadline and, indeed, were reported to be sending in reinforcements. Much of this history is important as background to current and potential developments in that torn country today.

The author deals extensively with the 1946 crisis in Turkey when Stalin began pressuring Turkey concerning Soviet claims to the provinces of Kars and Ardahan and for a revision of the Montreux Convention on the Straits that would have given the Soviets joint military control of this vital choke point. President Truman, following Acheson's strong advice that he must confront the Soviets now, decided that the time had come to challenge the Soviets and force them to retreat from their policies. Kuniholm concludes: "Soviet maneuvering over Iran and Turkey in 1946 significantly influenced the development of American foreign policy during the early years of the Cold War. The crisis in Iran crystallized the Truman administration's understanding of Soviet tactics. This understanding, in turn, conditioned the administration's reaction to Soviet notes delivered to Turkey in the fall of 1946" (p. 378). He tells us that the crisis over Turkey led to a reformulation of U.S. policy vis-à-vis the Northern Tier. The reformulation was occasioned by an awareness of the shortcomings of the United Nations, the pitfalls of accepting bilateral diplomacy between the USSR and its neighbors, and increased understanding of the requirement to be resolute in its policy toward the Russians.

Space limitations inhibit any discussion of one of the most interesting contributions of this volume, namely, the author's judicious treatment of the role and character of the prime players during the period, such as Stalin, Churchill, Roosevelt, and Truman. This study is in no way an exposé of this important historical period. It contains nothing polemical or sensational. It is distinguished by thorough research, superior scholarship, and calm assessment of the sources relevant to events of the time. It is an excellent book both in terms of the author's purpose and for scholars who wish to further explore and interpret the diplomacy of the great powers immediately after World War II.

The geopolitical situation along the Northern Tier has changed sharply since the period considered in this volume. This is really the meaning of the events taking place in Iran and Afghanistan in the past several years. The Northern Tier policy pursued by the United States in the period since the end of World War II was vital to preserving Western interests in the region. The loss of the West's geopolitical position in the area now threatens its access to the vital oil resources of the area on which U.S. and Western survival depends.

This study shows the reader that previous world

leaders in the West were aware instinctively of this fact, even if they were not always able to articulate or formulate our stake convincingly. Within the last several years, Western leadership has not seemed to grasp the salient importance of the Northern Tier and now we are striving—one hopes not in vain—to reverse the decline of our geopolitical fortunes in the region (for example, in Saudi Arabia). Many of today's policy problems occur, or are made more complicated, because they are dealt with in a vacuum of historical knowledge. The history in Kuniholm's volume might have helped present-day statesmen better understand what the Cold War in the Northern Tier was all about.

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SPENCER R. WEART. *Scientists in Power*. Cambridge: Harvard University Press. 1979. Pp. xiii, 343. \$17.50.

This is an interesting and well-written foray into, as the author puts it, "the region where pure science, joined by technology, mingles with every current of society" (p. viii). Its focus is on the nuclear physicists of the Collège de France led by Frédéric Joliot-Curie, Nobel laureate and prominent member of the communist resistance to the Nazi occupation. Founded on a wealth of primary source materials, it is of particular value to historians of France and historians of science, for whom it complements the earlier works of Robert Gilpin, Bertrand Goldschmidt, and Lawrence Scheinman. It is of wider interest as well, because the French scientists present a case study of men and women who had deliberately chosen "to build up science's funding, prestige, and organization so that precisely this sort of discovery [nuclear fission] could be made, appreciated, and exploited. These early decisions are to this day transforming the world's politics and economy. In this sense the scientists, whether they sat with government ministers or not, were in power" (p. 272).

Describing the expansion of that power, Spencer R. Weart opens with the efforts of the leaders of early twentieth-century French science to obtain and control funding for a foundering research community. He soon follows the uranium research of the interwar period and then explores in detail the work of Joliot-Curie, Hans von Halban, and Lew Kowarski in Paris in 1939-40, following both the latter to Cambridge and across the Atlantic during the war. The desperate French need for an independent source of energy provides the impetus for postwar chapters on Joliot's creation of an atomic energy administration capable of large-scale research under the control of the scientists themselves.

The key to the entire account is the terms of the

bargain between the state and its scientists. The scientists and mathematicians Weart presents—primarily the circle encompassing two generations of the Curie family—believed that political events were only “passing accidents” whose effects were small compared with those deriving from applications of scientific discovery. Politicians like Leon Blum, on the other hand, were convinced that politics were hardly secondary, for under capitalism “science would always be perverted to create wealth for the privileged few.” The two were partners: science creates wealth and socialism distributes it equitably (p. 34). The author, who is a trained physicist and director of the Oral History Project of the American Institute of Physics, does not resolve this conflict. He prefers to show how it affected the course of developments, namely by inducing the scientists to cater to the vision of material power harbored by the politicians in order to get the resources they wanted for their own ends.

By presenting matters largely from the perspective of the Curie circle, Weart achieves an important immediacy and clarity in dealing with the complexities of scientists facing and making major decisions. This approach also has drawbacks. Because his subjects were predominately progressive, the French left is well and sympathically presented, while the right seems always hovering just out of view. Looking at science through French eyes produces traces of cultural ethnocentrism, such as leaving a number of important German researchers unnamed or designating the Cavendish Laboratory at Cambridge as “the Radium Institute’s great rival for leadership in nuclear physics” (p. 38) rather than the other way around. Weart’s sharp focus on the science-politics nexus surrounding Joliot-Curie also leads to the gradual diminution of the role of his wife Irène Curie until she seems nearly forgotten.

Yet the story is compelling and revealing. Weart is aware that he has raised many more questions in this book than he has answered, particularly regarding the efforts by scientists to control the consequences of their research. But he does make it clear that the problems and opportunities arising from the results of their labors are intimately bound up with the structure of a given society and its organization of science, and he enriches the discourse between political historians and historians of science in attempting to understand the relationship between science and politics in the twentieth century.

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DONALD J. DIETRICH. *The Goethezeit and the Metamorphosis of Catholic Theology in the Age of Idealism*. (Euro-

pean University Studies, series 23, number 128.) Bern: Peter Lang. 1979. Pp. 261.

The effort to incorporate philosophic idealism into the mainstream of traditional Catholicism by German theologians at the turn of the last century is the theme of this monograph. Roman Catholicism recognized in the new intellectual impulse a valuable resource against the encroachments of the Enlightenment; at the same time idealism contained within itself a more subtle threat of secularization. Catholic theologians risked intellectual isolation if they repudiated philosophic idealism altogether, but they also realized that the essence of Christianity—the reconciliation of man and God through Christ—could not be derived philosophically and that intellectual accommodation exposed them to the possibility of condemnation by the official church. In fact, the works of a number of theologians comprised by this study were consigned to the papal Index. Georg Hermes, Johann Michael Sailer, Johann Sebastian von Drey, Johann Adam Möhler, Franz Anton Staudenmaier, Johannes von Kuhn, Johann Baptist von Hirscher, and Anton Günther are the theologians whose achievements Donald J. Dietrich selects to illustrate the precarious task of assimilating a new heritage while defending the integrity of an old one.

Historicism was unquestionably the single most rewarding perspective that Catholic theology adopted from contemporary idealism. Responding to the Hegelian system, Möhler, for example, insisted with his Tübingen predecessors that God’s message to mankind was eternally the same but that “the way men believe, understand, scientifically analyze, and systematize are accidental changes” (p. 104). The dogmatic expressions of revelation change; the essence of the divine message is constant. Kuhn as well as Staudenmaier discovered in the Hegelian dialectic a means of vindicating the Catholic church as a living tradition and community for the purpose of transmitting Christian truth in counterdistinction to the exclusive Protestant reliance on Scripture, a reliance which they considered dangerously open to subjectivism. *The Goethezeit and the Metamorphosis of Catholic Theology* surveys the individual writings of the theologians discussed in painstaking detail, a welcome service to American scholars for whom much of this material would be inaccessible.

Nevertheless, there is something arbitrary in Dietrich’s method. In his preface the author deplors the eclipse of intellectual history by social history and expresses his own conviction that “intellectual history uncovers the necessary knowledge of meanings and values which has aided individual and group perceptions that have affected actions. If intellectual history without social substance is empty,

then social history without ideas is blind" (p. 11). Dietrich describes the late eighteenth-century political and cultural origins of the Catholic revival; his concluding analysis of Hirscher and Günther recognizes the dangers of discussing theology in a vacuum and takes account of the Catholic response to the revolution of 1848, the critique of capitalism, and the belief that contemporary social issues required the moral intervention of the church. But the intervening discussion does in fact isolate theology not just from the political and social process but also from such developments as the reform of Catholic pedagogy and the evolution of the popular polemic confessional press. The extraordinary emergence of the Catholic church as a vital force in post-1848 Germany was rooted in the recruitment of a socially heterogeneous priesthood and the dynamic contact with an economically diverse laity during the decades following liberation from Napoleon, as well as in the sensitivity of Catholic theology to contemporary intellectual currents. The importance of Dietrich's thesis would be more evident had he provided a richer context for the theological developments he describes.

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ANTONIO ACERBI. *La Chiesa nel tempo: Sguardi sui progetti di relazioni tra Chiesa e società civile negli ultimi cento anni.* (Scienze Religiose, number 4.) Milan: Vita e Pensiero, Università Cattolica del Sacro Cuore. 1979. Pp. vi, 315. L. 12,000.

This analysis of the century-old struggle by the Catholic church to "enter into a dialogue with the world" is a limited attempt to carry Ernst Troeltsch's *Die Sociallehren der christlichen Kirchen und Gruppen* (1911) beyond the eighteenth century. Antonio Acerbi teaches church history at the Università di Lecce, but his interests in jurisprudence and theology often overshadow the writing of history in this work. *La Chiesa nel tempo* is close to Troeltsch's intellectual-theological model of religious history and far from the typical French model of the social history of religion.

Acerbi gives us some historical models of Catholic thought on the relations between church and society, with his emphasis falling on the historicity and evolution of Catholic thought since Leo XIII. He accepts the concept of change as well as the permanent foundation of Catholic theology and the basic truth of the Catholic Weltanschauung; but he also acknowledges that when the church operates as *Lumen Gentium* (title of a dogmatic constitution issued by Vatican II) in a historical environment, it does not escape temporal contamination by cultural and theological elements of the period. There is a

body of theory and praxis that guides the church in making choices in its relations with civil society; these paradigms—Acerbi calls them "*progetti storici*"—change over time.

Acerbi's book begins with the *progetto storico* concocted by Leo XIII and ends with the tardy acceptance of the validity of the principle of religious freedom; this is the thinking man's guide to Catholicism between the *Syllabus of Errors* (1864) and *Dignitatis Humanae Personae* (1965), an analysis of how the church, especially the papacy, finally limped into the nineteenth century as it came to grips with the unique problems of the twentieth.

Nearly a third of *La Chiesa nel tempo* deals with the heroic and utopian efforts of Leo XIII to end the state of war existing between church and society under *Pio nono*. Leo could not have a dialogue with his time because his ahistorical view of Christianity did not allow for any genuine relations between the temporal order and the perfect and transcendent church. But the *progetto storico leonino* did leave a positive legacy: a nucleus of basic ethical principles based on a recognition of the value of the human person. There is a direct line of descent from Leo's plan for social regeneration to recent church policy of solidarity with the world's poor.

In the period between the two world wars there were two conflicting Catholic *progetti storici* on relations between the church and civil society. One was essentially French Christian humanism, mostly the Thomistic creation of Jacques Maritain, who argued that political democracy is the natural habitat of the church and justified a vastly enlarged role for the laity. Confronting this French *progetto* was that of Pius XII, who concluded Pius XI's plan for a systematic body of Catholic social doctrine founded on natural law. Influenced by the social metaphysics of Gustav Grundlach and by the conservative political theory of Carl Schmitt, a teutonically inspired Pius XII built his *progetto* on the ideal form of justice and on eternal law; the ahistorical spirit of Leo XIII was hovering near. Pius XII emphasized the role of the hierarchy more than that of the laity; authority, not community, provided inspiration for this *progetto*. These competing *progetti* do have points of agreement, especially in the acceptance of political democracy—one of the unintended consequences, perhaps, of the church's unalliance with the West to fight off the communist threat, although the potentially disastrous consequences of flirting with totalitarian regimes had become all too clear by the 1940s.

About a third of this book is on Vatican II (1962–65) and some of its revolutionary work. Special attention is given to *Gaudium et Spes*, the council's pastoral constitution on the church, a document much influenced by John XXIII's encyclicals and the French *progetto*. A last chapter, centered on an anal-

ysis of *Dignitatis Humanae*, the council's declaration on religious freedom (a symbol for Acerbi of the general evolution of the church in the twentieth century), shows that Troeltsch was wrong in believing that the church could not accommodate itself to modern culture with its devotion to religious liberty. Vatican II made it clear that the church has renounced its role as a guide for society. Acceptance of the principle of religious liberty signified recognition of the historicity of Christianity; it was also evidence of considerable U.S. influence, not unrelated to the role of the U.S. as the self-anointed champion of Western freedom. An important role remained for the church: to be the *sacramentum unitatis* of humanity, without Catholic cultural or social hegemony. The church was close to Troeltsch's conclusion on the impossibility of the "Kingdom of God upon earth as a completed social organism. . . . the Kingdom of God is within us. . . . The final ends of all humanity are hidden within His Hands."

La Chiesa nel tempo is a useful book; it has no bibliography (although there are adequate references in copious notes) and a rotten index. It would still be premature to hail an Italian Troeltsch.

HARRY W. PAUL

University of Florida

WILLIAM B. COHEN. *The French Encounter with Africans: White Response to Blacks, 1530-1880*. Bloomington: Indiana University Press. 1980. Pp. xix, 360. \$22.50.

A literature of cross-cultural images has grown up in recent years, dealing with the ideas and attitudes one society has of another. Some studies concentrate on the professional attitudes of an elite, like Edward Said's *Orientalism*. Others focus on ideas held by a broader, if less articulate, public. Still others center on a particular subject or policy. William B. Cohen's study of French thought about Africans is more concerned with elite than with mass opinion. It concentrates on three recurrent themes. One is a policy question—what to do about the slave trade and slavery? A second theme has to do with the intellectual underpinning of French imperialism in Africa and the Americas—ideas designed to justify imperialism and to guide imperial policies. A third is the matter of race—the French effort to assess the role of race in human society, particularly the assessment of sub-Saharan Africans.

Of these themes, French racism is the independent variable influencing French attitudes toward slavery and imperialism. In the background is the old myth that the French had less race prejudice than either English or Americans, which is demolished easily enough. Cohen is more concerned with growth and change in French thought about race. He finds its roots in the Middle Ages, where Euro-

pean ideas about sin and retribution helped to further a certain anxiety about what Europeans might be like, were it not for the fetters of Christianity and custom. The initial image of Africans as lazy, animalistic, and sexually lustful came from a projection of these fears onto others who had the most obviously different physical appearance. It was not based on wrong evidence, but on no evidence at all, although future observers of Africans took it as a point of departure.

The main line of racist thought in the eighteenth and nineteenth centuries is, of course, European, not specifically French. The consideration of slavery and the slave trade or of the practicality of emancipation had an Atlantic framework of intellectual interaction. Marking out the French case for special attention, however, makes it possible to highlight points that have sometimes been overlooked. Cohen shows, for example, that the anti-Africanism of Frenchmen who gathered their evidence in the Americas was much stronger than it was among those who had visited Africa.

French racism in the nineteenth century also took a somewhat different course from that found in Anglophone countries. The prestige of biological studies was greater in France than in Britain in the first part of the century, which encouraged French practitioners of scientific racism to look for a physiological base—to a degree found in Britain only in its last decades. (One continuing consequence was the French understanding of the content of *anthropologie* as mainly physical anthropology, not social or cultural.) At the time, this laboratory basis for racist thought tended to separate even more strongly the opinions of those who knew Africans as living and social human beings from those who knew them only as specimens or theoretical constructs. French explorers and administrators in the late nineteenth century therefore came to provide a significant antidote to the vitriolic theoretical racism of the metropolis, although many of them had also absorbed a part of the poison.

Cohen's book is not only an admirable and well-written study, making an important addition to this body of literature, it is also beautifully designed and printed by Indiana University Press—at a time when most academic presses have stopped trying. Its most important failing is probably inevitable with studies that try to report what people thought about reality. It is all too easy for the author to know what is myth and what reality but not to tell the reader. Every now and then, Cohen himself is taken in by his authorities, as when he transports orangutans from their proper setting in Southeast Asia to the coast of Angola (probably a case of mistaken identity for chimpanzees or lowland gorillas).

PHILIP D. CURTIN

Johns Hopkins University

JONATHAN R. ADELMAN. *The Revolutionary Armies: The Historical Development of the Soviet and Chinese People's Liberation Armies*. (Contributions in Political Science, number 38.) Westport, Conn.: Greenwood Press. 1980. Pp. x, 230. \$22.50.

Despite their comparable philosophies and origins the Soviet Red Army and the Chinese People's Liberation Army (PLA) have played quite different roles in their nations' development. The PLA was deeply involved in party decision making throughout the 1950s and 1960s; in the 1920s and 1930s the same could not be said of the Red Army. When purges of PLA leaders occurred, they were neither as sweeping, nor as severe, nor as lasting as comparable purges of Soviet army figures. The PLA was more involved in economic development projects than was the Red Army, and, unlike the Red Army, it was often held up to the nation as a model for all. Jonathan R. Adelman seeks the reasons for these differences.

He first examines and dismisses several hypothetical explanations: the level of military technology (both were unsophisticated); the international political environment (both were hostile); the level of economic development (both were relatively undeveloped at the time of their civil wars). It was, Adelman says, the differing gestation periods of these two communist armies that explain the different roles they were to play in the post-civil war lives of their nations.

The Red Army, quickly assembled in the winter of 1917-18, contained a potpourri of elements with but a small inner core (5 percent) of communists. It experienced high desertion rates, numerous acts of treason, a modest ability to mobilize men effectively for combat zones, and, frequently, a poor combat record. Consequently, Soviet leaders sought to centralize their control of the army, and after the Civil War they sharply cut back its size and role. Its commissars served as watchdogs, rather than as trusted colleagues, and a secret police army was for some time its rival for resources and in capabilities.

Not so with the PLA. The many years of gestation (1927-45) allowed the Chinese Communist leadership to train its own officer corps, purge dissidents, and emerge in 1946 with an able force (24 percent of which was Communist) for the final struggle with Chiang. Close party-army relations left no need for the centralization practiced by the Soviets, and five regional PLA field armies operated with considerable initiative. Desertion and treason were infrequent. The PLA fought well. The secret police was not a significant presence, and the commissars served more of an educative than a watchdog role.

With such a record, it is not surprising that the Chinese Communist leadership would give the PLA

substantial roles to play in the nation's political, economic, and cultural development.

Adelman builds a convincing case and can be faulted only for relatively minor matters. The book is repetitious; some was necessary, but there is too much of it. He seems unaware (pp. 22, 213-14) of some recent studies of the military in *fin de siècle* China, although the use of these studies might not have altered his case substantially. Text and tables on pages 119 and 121 do not agree, but the discrepancies are not serious. One would have liked him to comment on the PLA's role in the purge of the "Gang of Four," as this would *seem* to provide further evidence of his thesis, but one must not tell authors what to say, especially when they have done as well as Adelman.

PETER KARSTEN
University of Pittsburgh

ANCIENT

OSWYN MURRAY. *Early Greece*. (Fontana History of the Ancient World.) Atlantic Highlands, N.J.: Humanities Press or Harvester Press, Brighton, Sussex. 1980. Pp. 319. \$32.50.

ANTHONY SNODGRASS. *Archaic Greece: The Age of Experiment*. London: J. M. Dent and Sons. 1980. Pp. 236. \$27.50.

In the past five years, study of the seventh and sixth centuries of ancient Greece has become eminently fashionable. Interdisciplinary research has produced sufficient evidence to trace the institutional and intellectual developments of an age claimed to be "perhaps the most important period in Greek history." The story of this period—the Archaic Age—has been told in seven major studies published between 1975 and 1979, and two new studies have just been added to the list. The author of one of the most recent books is bold enough to express the suspicion that there may have been enough books on the subject before his own appeared. That concern is unjustified: the new studies describe the Archaic period for audiences not previously addressed and differ not only from the earlier publications but also from one another.

Oswyn Murray's *Early Greece* is a volume in the Fontana History of the Ancient World, which has the dual purpose of providing an up-to-date account of major periods and of presenting the methodology employed by scholars of each particular age. Murray has fulfilled both tasks admirably. In sixteen chapters and 279 pages of text, he moves chronologically from the end of the Dark Age to the conclusion of the Persian War. In straightforward

language, the author sets the story of Greece in a broad perspective geographically and intellectually, emphasizing the role of concepts in history, the unity of the eastern Mediterranean, and the significance of social customs.

General tendencies and particular events are interwoven so that the reader learns, for instance, of Solon's reforms in the context of codification of laws throughout the Archaic Greek world. The picture that emerges is, on the whole, an orthodox interpretation. Still, Murray is not reluctant to state his opinions on controversial matters nor does he shrink from difficult subjects—the motivation of Cleisthenes the Athenian, the Greek attitude toward sexuality. Presenting the evidence fairly, his argument is clear and reasonable.

Technical production has been careful and there are very few errors. Only eight pages of illustrations are included in the center of the book; more would have been helpful. Six maps and a date chart, discussion of the primary sources, and suggestions for further reading complete the book. *Early Greece* is a boon for instructors although it is not tailored for the novice. The title is somewhat misleading—early Greece is more than the Archaic period—but this does not diminish the quality of the study.

Anthony Snodgrass has written what might best be termed a series of essays on the Archaic period. Rather than a detailed account of various regions of Greece, the six chapters of *Archaic Greece: The Age of Experiment* describe major characteristics—the human factor, for example, is the focus of the first chapter and economic realities the subject of the fourth. Yet “essays” may imply limited themes, an impression that would be entirely false. Following Colin Renfrew's method of analyzing the multiplier effect, Snodgrass masterfully demonstrates the nature of interactive change, developing a unified interpretation of the entire period. The chapters reveal the wide ramifications of developments in a particular sector of society in an age depicted as “a long era of restrained experiment, bounded at either end by shorter periods of more hurried, at times almost feverish innovation” (p. 217).

Snodgrass, an archaeologist, employs the tools of that discipline to provide new insights. Examining the types and quantities of metallic dedications at sanctuaries, for instance (p. 52 ff.), he describes major social, religious, and technological change. His conclusions are bold, yet undogmatic and informed by common sense. He is willing to present new hypotheses—the importance of Dark Age pastoralism—or to challenge orthodox positions—the role of the Persian Wars in creating artistic change. The perspective is broad, drawing on parallels from other cultures, the interaction between Greece and other contemporary societies, and insights from other disciplines.

The book's format is excellent. There are three sections of fine plates with references to specific illustrations in the margins of the text; fourteen additional figures are interspersed throughout the text. Typographical errors are virtually nonexistent. In every sense, Snodgrass's account is a fitting study to stand at the end of a list of books on Archaic Greece. Drawing together the results of multidisciplinary investigation, it presents a brilliant analysis of a culture in the process of growth for well-versed readers.

C. G. THOMAS

University of Washington

PAUL CARTLEDGE. *Sparta and Lakonia: A Regional History, 1300–362 B.C.* (States and Cities of Ancient Greece.) London: Routledge and Kegan Paul. 1979. Pp. xv, 410. \$22.50.

Paul Cartledge's attempt “inevitably provisional, to map out a new kind of history of ancient Sparta” (p. xiii) has produced an idiosyncratic book, rather uneven, narrowly focused, and sometimes disappointing. The reader will find little discussion of the Spartan social or educational system or of the dynamics of internal politics in the classical period. By contrast, there is a wealth of useful information about the topography and resources of Lakonia (and, to a lesser extent, Messenia) and an emphasis on the relationship between the outlying regions controlled by Sparta, where helots and *perioikoi* lived, and the Eurotas valley where the Spartiates dwelt. The book's most useful contribution, in fact, may be in its attempt to describe settlement patterns throughout the region and to assess their importance to the vital interests of Sparta's security and prosperity. Indeed, in part 3, “Classical Lakonia,” the primary emphasis is on warfare and the implications of a dwindling citizen population for Sparta's role in external affairs.

It is very difficult to decide for whom this book is intended. Cartledge's explanation of technical terms such as *fibula*, *skyphos*, *aryballos*, and *proxenos*, sure to be familiar to any except the most elementary student of classical archeology or history, suggests a general audience with little background in the subject. But his assumptions at other points, for example, about the nature of the Spartan *agoge* or the identity of Aristophanes of Byzantium, suggest the contrary. In part 2, “Preclassical Lakonia,” Cartledge is at his best in handling the archaeological material, of which he obviously has superb command. His attitude to the literary evidence for this period, although avowedly skeptical, is less satisfactory. While attempting (not very adroitly) to skirt the “Lycurgan question,” he accepts the traditional dating of the First Messenian War (735–715), based

on Pausanias, and does not shirk from suggesting the date and circumstances of the Great Rhetra.

Almost half the book is devoted to the theme of oliganthropy and its consequences for the decline of Sparta's position in Greece after Leuctra. Undue space is devoted here to unprofitable speculation about the status of helots and *perioikoi* and unnecessarily detailed narrative of military affairs. Cartledge declines to consider Spartan internal politics to any degree, and this methodological flaw weakens the book but perhaps explains his failure to include any reference to Poralla's *Prosopographie der Lakedaimonier*. Given his emphasis on population changes and demography as the key to Sparta's failure (an insufficient explanation), however, the absence of Cavaignac's article on population in the Peloponnesus (*Klio* [1912]) is curious. Cartledge's suggestion that the perioikic communities formed independent *poleis* whose relations with Sparta provided the model for the later Peloponnesian League is engaging if unprovable, but the hypothesis that the Athenians only discovered that the Messenian helots were Greeks on the occasion of their assistance to the Spartans against these "rebellious slaves" in ca. 463 is untenable, particularly in view of the presence of helots with the Spartan forces at Plataea in 479. In many of the questions discussed, Cartledge has, I fear, been no more successful than other scholars in penetrating the veil of secrecy and obscurity that, as Thucydides notes, the Spartans drew about themselves and their society in the fifth century.

CHARLES D. HAMILTON
San Diego State University

JEAN W. SEDLAR. *India and the Greek World: A Study in the Transmission of Culture*. Totowa, N.J.: Rowman and Littlefield. 1980. Pp. xxi, 381. \$30.00.

The influences of India on the Greek world are even greater than those enumerated in this book. This study is thus rather like an archaeological slice that allows us to partially examine aspects of religion, one of the most important areas of India's influence on the Western world. It is also a reflection of Western images and stereotypes, which for centuries have focused on India's religions.

There are certain religious perspectives that are widely held to be "Indian" in character if not in origin. Among these are the transmigration and reincarnation of souls, asceticism and celibacy, the law of karma, the idea that the individual soul is identical with the world soul, religious vegetarianism, and yoga. This study by Jean W. Sedlar carefully analyses the writings of Greek and early Christian thinkers where they may show influences of these and other related Indian ideas.

For the most part, she concludes that the likelihood of influence is marginal; the ideas differ in important essentials; and the probabilities of contact are remote. The universal features of the concepts considered make it just as likely that similar ideas developed independently. She quite correctly indicates that "what the Greeks actually knew about India therefore was not very much and not very accurate" (p. 13). This assessment applies with even greater force to our own times.

But this study is more than an analysis of Indian religious ideas found in early Greek thought. There is an exciting section on the Indian parallels to the Gnostic theologies of the early Christian centuries and the relationships between Gnosticism and similar important ideas in Christianity and Judaism. Buddhist literature is also examined, and Sedlar concludes that the life story of Christ is probably borrowed from that of the Buddha "in the instances of the aged saint's prophecy, the temptation, the walking on water, and the feeding of the multitude, which occur in the New Testament itself. Likewise in the Apocrypha, various parallels with Buddhist stories suggest dependence. . . . In all these cases the Buddhist versions are probably the originals" (p. 122).

While it is generally known that St. Augustine, one of the most influential of Christian theologians, was a Manichaean before converting, the Indian origins of Manichaeism are little known. Sedlar here details the Indian-related ideas and institutions that influenced Christianity into the fourteenth century, some of which carry over into the present day. However, in common with many who write on Indian religions, she unfortunately distorts and exaggerates what she calls "the life-negating attitudes of Hinduism, Jainism and Buddhism" (p. 227).

This book is a sober, bold, well-documented study. It is a major contribution to its field and to our understanding of the significant and often hidden ways in which Indian thought and ideas have influenced our heritage and beliefs. It also makes us more aware of the long history of ignorance that has existed in the West about India and its contributions.

J. W. SPELLMAN
University of Windsor

GEOFFREY RICKMAN. *The Corn Supply of Ancient Rome*. New York: Oxford University Press. 1980. Pp. xii, 290. \$54.00.

This fascinating book deals with what the author calls one of Rome's two great achievements in supplying the city's needs, the other being the aqueduct system. Geoffrey Rickman begins with a general survey of the problems involved for any urban

center: the human diet, the growth of population, the difficulty and cost of transport by land or water, and the conditions of storage. He expects to show the "slow but ever-growing involvement of the state" in production, transport, and distribution. Thus he begins the story in the Roman republic when shortages played their part in social upheaval and in the suppression of piracy in the Mediterranean, goes on to the transitional period with the creation of the *praefectus annonae*, and next deals with the early empire, in which this prefect steadily acquired power. (Along the way he notes, although without reference to C. G. Starr, the relevance of the Roman navy as protector of the lifelines.)

He then provides chapters on "the corn lands" (Italy, Sicily, Sardinia, Spain, Africa, Gaul, Egypt, and the eastern provinces), on transport, storage, and prices (some significant data are no longer extant). The grain distributions are assessed and found unrelated to poverty or any "means test." The emperor simply wanted to strengthen the urban plebs. A final chapter deals with the late empire, where water-powered mills are described as introduced under state control in 398 (p. 206). One wonders why so late, when Constantine was already using water thus near Arles.

The study reminds us that the Roman world was not held together just by troops and propaganda. Rickman's prefatory quotation from Seneca to his father-in-law, "Reflect that you were not aiming . . . in all your training in liberal studies, at this, that it might be safe to entrust many thousands of modii [of grain] to your charge," surely suggests that more studies like his on the "facts of life" should be welcome.

At only one point does he seem to contradict himself, when he says (p. 179) that in A.D. 6 Augustus "gave to the poor as much grain again as they received from the state." His basic argument has been that the "corn distributions" were *not* to the poor, and the references given here to the *Res gestae* and Dio do not mention or imply any alteration.

Eleven appendixes at the end deal in detail with aspects of the evidence for the situation, although the study itself ends with "the final paradox" of the fourth-century emperor, now become "a prisoner in the system that had been created" (p. 208). Where it should have been halted remains an open question.

ROBERT M. GRANT
University of Chicago

EDWARD CHAMPLIN. *Fronto and Antonine Rome*. Cambridge: Harvard University Press. 1980. Pp. 185. \$14.00.

Edward Champlin's book on Fronto is the latest in a line of slender volumes that consider the relations of significant figures in the Roman empire with their society and milieu. Following upon G. W. Bowersock's *Augustus and the Greek World* (1965) and *Greek Sophists in the Roman Empire* (1969) and C. P. Jones's *Plutarch and Rome* (1971) and *The Roman World of Dio Chrysostom* (1978), Champlin offers discussion of a Latin writer (here is a novelty) who had a unique relationship with the imperial house as tutor of two emperors, Marcus Aurelius and Lucius Verus.

A student of Fronto labors under the handicap of dealing with a wretched text, preserved only on a palimpsest and vitiated by numerous lacunae. Consequently, much that is argued is of necessity conjectural, but Champlin handles his material sensitively and intelligently. The picture that he draws of Fronto is convincing, but not, to me, appealing.

Although consul, Fronto was not a political figure important in the government of empire. He never commanded an army, he never governed a province. But he moved on the highest levels of intellectual intercourse among both Greek and Latin speakers, and his eminence in oratory was unquestioned. His influence on Verus, particularly, was not always what he desired. He was an arrogant, petty man, occasionally an "intriguer outmaneuvering his rivals at court, and as unctuous a flatterer as any" (p. 115).

There are eight chapters: "Africa," "Italy," "Literary Society at Rome," "The Man of Letters," "The Lawyer," "The Senator," "The Friend of Caesar," "The Teacher of Emperors." It would have been useful to have added a chapter on the historical background of the years 138 to 180 and to have underscored the dramatic change from a period of largely profound peace to one of difficult and devastating wars. I would have welcomed more on some other major figures of Antonine Rome, such as Galen, who contributed much to the character of the age.

As always, one can disagree with points here and there. I find Champlin excessively harsh on Hadrian; he condemns Hadrian for allowing the armies to be demoralized by soft life (p. 116)—hardly. He gives Fronto credit for popularizing the archaistic movement (p. 45) but later admits that Hadrian may have influenced him (p. 94), as it must have been. His dating of the *Ciris* (p. 57) is not supported by the latest editor of the poem, R. O. A. M. Lyne (1978). But disagreement on details should not condemn the whole. Champlin has given us a good book, sane and searching, that enables the reader to learn a great deal about Fronto, perhaps even more than he will wish to know.

HERBERT W. BENARIO
Emory University

MEDIEVAL

PHILIPPE CONTAMINE. *La guerre au moyen âge*. (Nouvelle Clio: L'Histoire et Ses Problèmes, number 24.) Paris: Presses Universitaires de France. 1980. Pp. 516.

During the past fifteen years Philippe Contamine has established his reputation as one of the finest military historians in the world. His newest work fulfills the highest expectations in offering the bibliographic survey and outline of the state of the art in a manner now traditional in the "Nouvelle Clio" series. The impressive bibliography of over one thousand entries, which seems to have missed nothing, leaves no doubt about the erudition of the author, and the remaining four hundred pages of analysis and discussion demonstrate that these materials were critically evaluated as well as noted. In his analysis, Contamine regularly suggests where scholarship has yet to go. A particularly good example is his remarkable chapter, "Pour une histoire du courage," where he suggests the need for further study of the psychology of war and proceeds in the next twelve pages to offer a provocative discussion of how such a study might be conducted.

After the preliminary bibliographic section comes a general survey of what is currently known about medieval military history, dealing with the barbarian era, the feudal era, the High Middle Ages, and the late Middle Ages in chronological sequence. The most suggestive remarks appear in a separate third section, "Thèmes et perspectives," where armament, artillery, military thought, psychology, the relations between war and the organization of society, and legal and religious theories about war are analyzed. In these later sections Contamine continually follows up general remarks with detailed analysis of specific examples drawing on chronicles, contemporary records, and imaginative re-evaluation of sources, as in his discussion of the origins of the word "artillery" on pages 332-33.

The apparent ease with which Contamine deals with the multifaceted problem of war makes the circumspection with which he writes all the more frustrating. In some ways he has been overwhelmed by the magnitude of the topic, and he is reluctant to comment on controversial topics. His discussion of the Brunner thesis and its subsequent fate is the best available, but he stops short of a final evaluation. Further, the separation of general discussion and topical analysis may be counterproductive. Had late medieval warfare and the evolution of artillery been discussed together, the impact of technology on late medieval tactics and strategy would have been better understood by the reader. These shortcomings are minor, however, in a work that by

its very nature will be more often consulted than read in its entirety. It is always easy for a reviewer to imagine a different sort of book having been written. It remains difficult for this reviewer to imagine a better one.

PAUL SOLON
Macalester College

JOHN BOSWELL. *Christianity, Social Tolerance, and Homosexuality: Gay People in Western Europe from the Beginning of the Christian Era to the Fourteenth Century*. Chicago: University of Chicago Press. 1980. Pp. xviii, 424. \$27.50.

Although this remarkable book is an irreducible treasure of information about sexual attitudes, its conclusions can be briefly summarized. There was general acceptance of homosexuality among the ancient Greeks, Romans, Jews, and early Christians. Most of those texts that have been interpreted as being antagonistic to homosexuality are in fact aimed at something else, such as homosexual prostitution, passivity, or rape. Although hostility toward homosexuality increased briefly during the patristic period, it receded in the early Middle Ages. The twelfth century was one of widespread acceptance and even praise of homosexuality. In the thirteenth century, however, Western civilization assumed the intense antipathy for homosexuality that has been its common public posture ever since.

John Boswell presents these shifts as part of the history of tolerance, and his explanations for them are speculative. He suspects that the breakup of urban society in the patristic period contributed to the rise of intolerance. He has no explanation for the dramatic shift of the thirteenth century, apart from saying that the origin of the change was more popular than clerical and that the new hostility was not caused by the Christian Bible, which had been there all along.

Boswell argues convincingly that only in the thirteenth century did homosexuality come to be singled out as an evil deserving special opprobrium. Only partially compelling are his arguments that most earlier Jews and Christians had no real objection to homosexuality. I am not convinced that the account in Genesis of Lot and the Sodomites is intended to condemn *only* the sin of inhospitality. Nor am I convinced that the phrase "beyond nature" was morally neutral to St. Paul because he used it with reference both to homosexual acts and to the salvation of the Gentiles.

Boswell proves that writers in the West have been marvelously ambiguous in their writings about what is natural and unnatural in sexual relations, but I am not certain that their efforts were as senseless or as fruitless as Boswell seems to believe. Bos-

well is not entirely successful at keeping his own values from intruding. Undefended moral views of others are occasionally "prejudices"; his own (for example, against sexual "exploitation") are not. But given the rabid animosity the subject has inspired in other writers, this study is admirably dispassionate and objective.

Despite these and other reservations, let me recommend this book as a splendid piece of scholarship. Boswell argues responsibly, plausibly, and with formidable erudition. He moves with ease through ancient, medieval, and modern languages, forcing his readers to look at dozens of familiar passages with new eyes. To his and his publisher's credit, extensive texts are presented in the original and in translation.

This brief review can only hint at the richness of this provocative book. It is fundamental for future studies of sexual attitudes in the West.

JOHN C. MOORE
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CARLO SERVATIUS. *Paschalis II. (1099-1118): Studien zu seiner Person und seiner Politik.* (Päpste und Papsttum, number 14.) Stuttgart: Anton Hiersemann. 1979. Pp. xii, 401. DM 160.

In this study of Paschal II, Carlo Servatius declares his intention to fill in the gaps in our knowledge of this important reform pope. As a result, he eschews the route of a full biography or an investigation of Paschal's pontificate in all of its aspects to concentrate instead on his life and career previous to his election, his legateship in Spain, and the relations of the church with the German empire. Most notably, he has decided not to deal here with Paschal's important involvement in the investiture controversy in France and England. While Servatius provides a careful and well-documented study of his subject, there is a certain disappointment that he did not accept the challenge presented by a complete biography.

Paschal II has long been an intriguing figure. His extraordinary proposal for the solution of the conflict with the German empire over investiture of clerics by lay lords was the most radical ever undertaken by a medieval pope. It called upon the German emperors and nobility to renounce any role in the internal affairs of the church and for the bishops and abbots to resign all rights and properties held from the crown or other secular authorities and to focus their attention completely on their spiritual concerns. When the storm broke so furiously over this proposal that it threatened the imperial coronation of Henry V, Paschal, taken prisoner, recognized the continued right of the German monarchy to invest the clergy with the symbols of their spiri-

tual office and conferred the imperial crown on his captor. Once free, isolated from many in the reform party by his acquiescence to imperial demands, Paschal was forced to renounce the "privilegium" to Henry V at the Lateran Synod of 1112. No wonder historians have praised his insight, condemned his weakness, stressed his clever political sense, and called him a fanatic without ever feeling confident that they really understood this complex figure, whose *vita* in the *Liber Pontificalis* characterizes him as a "holy man."

If Servatius contributes substantially to a better understanding of Paschal II, it is by his meticulous and well-informed discussion of numerous aspects of Paschal's pontificate, which enables us to place it more securely into the development of the post-Gregorian papacy. Yet the final figure that emerges not only suffers from a lack of definition similar to that presented by earlier scholars but also raises new questions that the author seems unable to answer. Servatius suggests that Paschal was not an intransigent fanatic and that his radical proposal did not represent deep dissatisfaction with the church's ownership of property, but he fails to provide a more positive picture of Paschal and his work. His conclusion that Paschal was less a pathbreaker and model than a follower and heir leaves much still in question. In my own view, the decision to limit the scope of the present work resulted in a narrowing of the potential for dealing with Paschal in a comparative framework or for seeing his failures in the context of his role in the controversies with England and France. While Servatius does refer to the broader picture to enlighten this aspect of the problem, his efforts are insufficient and limit the significance of an otherwise first-rate work.

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CARL J. DAHLMAN. *The Open Field System and Beyond: A Property Rights Analysis of an Economic Institution.* New York: Cambridge University Press. 1980. Pp. viii, 234. \$27.50.

This is a highly theoretical, but not entirely un-historical discussion, from the viewpoint of the economics of law and property right, of the English common or open field system. It seeks a single theory to "account for private ownership of the arable, but collective ownership of grazing grounds, individual decision making with respect to many aspects of cropping, but well defined limitations imposed on this decision making by social rules and regulations, the striking scattering of ownership, and its antithesis, consolidation in the enclosure of the village, the reasons for enclosure and its timing, and the origins of the system" (p. 5).

Carl J. Dahlman eventually decides that common rights in this system were not exactly communal or collective, but "collective exclusive" or "primunal" (p. 204), like those in a business corporation, and equally consistent with private wealth maximization. He accounts for most aspects of common fields in a similarly satisfactory and familiar way. The extinction of common rights, for example, was due to an extension of the market. But when it comes to the question of why the parcels were "so strangely scattered about" (p. 21), he concludes that "scattering was imposed on the villagers so as to make the collective decision making institutions viable and effective" (p. 144).

Not everyone will find this better than the risk aversion theory. In some places the author's stylized presentation of reality betrays a lack of knowledge. He exaggerates both the extent and the speed of the adoption in common fields of innovations pioneered in severalty and fails to see that convertible husbandry permitted both tillage and pasture in the selfsame close under one and the same property right. He fails, too, to address the one burning question, and one not improper for a student of the economics of property rights, namely, the causes for the division of landed property into the hands of a multitude of small proprietors.

Yet it is manifest that although one theory will account for the same system of land use obtaining in both common fields and demesne farms in severalty, all the carefully rehearsed explanations of scattering in common fields assume the fragmentation of landed property, which requires a separate theory of its own. All said, however, this book deserves high praise for its power to stimulate and clarify.

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NICHOLAS PRONAY and JOHN TAYLOR. *Parliamentary Texts of the Later Middle Ages*. New York: Clarendon Press of Oxford University Press. 1980. Pp. 230. \$44.00.

With this volume, Nicholas Pronay and John Taylor give scholars definitive editions of six texts relating to the late medieval English parliament. The texts are the Latin versions of the *Modus Tenendi Parliamentum*, Recensions "A" and "B"; the *Modus Tenendi Parliamentum et Consilia in Hibernia*; "A Rochester Account Concerning Disputes during the Parliament of 1321"; "A Colchester Account of the Proceedings of the Parliament of 1485"; and "A Draft of the Protestation of the Speaker (1504)." With the exceptions of the "Rochester Account" and the "Protestation" these texts have appeared in print before; a number of the earlier editions are, however, rather inaccessible. Each text is accompanied

by (1) an introduction, in which its historical significance and manuscript tradition are discussed, (2) a translation in the cases of Recension "A," the Irish *Modus*, and the "Rochester Account," and (3) a series of "Historical Notes" serving as unindicated footnotes to the text itself. In addition, five appendices provide lists of manuscripts and transcripts of the English and the Irish as well as the Scottish (Latin) *Modus* and notes on the early translations and the minor and printed versions of the English *Modus*. Because of the numerous sections, examination of the volume's organization before beginning to read it will alert the reader to the location of discussions of various topics.

Since the publication of their articles in the *English Historical Review* (83 [1968]: 673-88) and the *Bulletin of the Institute of Historical Research* (47 [1974]: 11-23), Pronay and Taylor's views regarding the manuscript tradition of the *Modus* and medieval interest in it as a legal rather than a political document have remained unchanged. They think the Irish *Modus* was created by 1419 from a French version under the probable supervision of Richard O'Hedigan, archbishop of Cashel.

In reality *Parliamentary Texts* is not a book that is meant to be read but rather is a small collection of edited documents, which have been placed together by Pronay and Taylor because they relate to aspects of late medieval parliamentary meetings. With the possible exception of the "Protestation," none of the texts is, in fact, an official document produced by a parliament. Questions may be raised about the selection of documents included in this collection, especially as an increasing number of new documents have come to light since the 1930s. The *Modus* is, however, the central concern of the editors, who have chosen the other texts because of their relationship to it. For example, the "Rochester Account" describes the parliament with which the creation of the *Modus* is associated, and the "Colchester Account" illustrates various parliamentary procedures noted in the *Modus*, how these procedures have developed since the 1320s, and how they have been misrepresented on occasion by the Chancery on the *Rotuli Parliamentorum*. Unfortunately, nowhere do Pronay and Taylor bring together the texts in a discussion of how parliament actually worked. This task is left to the reader.

As often happens nowadays, this volume is marred by a number of errors. Most of the typographical errors involve the printing of a wrong punctuation mark. But, in the case of the "Rochester Account," a comparison of the Latin text with the translation indicates inconsistencies in expanding given names, and in several instances individuals simply disappear, not appearing at all in the translation. Furthermore, American readers will be surprised by the cited locations of certain American li-

baries. These errors aside, the editors have produced fine editions of the six texts, which will aid scholars of parliamentary history in the future.

KENNETH G. MADISON
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grateful for what King has done, even if his publisher has priced his book out of bounds for large-scale classroom use.

MICHAEL ALTSCHUL
Case Western Reserve University

EDMUND KING. *England, 1175-1425*. (Development of English Society.) New York: Charles Scribner's Sons. 1979. Pp. vii, 214. \$20.00.

Edmund King states that what he has written is "intended less as a textbook than as a series of essays on a common theme, the unification of a single society" (p. xiii). In this aim he has basically succeeded, producing the kind of selective survey, written in an unhurried and uncluttered style, reminiscent (without quite the touch of mastery) of many of Christopher Brooke's works.

The early chapters are rigorously analytical, focusing on relations among peasantry, colonization, and cultivation; trade and urbanization; and the world of ideas, presented less as formal thought than as popular attitudes and legal precepts concerning property, status, and reputation. The last third of the book takes on more of a political and narrative approach. Neatly dubbing government as the great medieval "growth industry" (p. 137), King stresses the connections among warfare, taxation, representation, and political community. He thus views government as such as both the symbol and the catalyst of broader social change and integration. The more the notions of community and communities are studied, the more complex is the task of the social historian, and the more tenuous (if not dangerous!) are his generalizations. King ably generalizes where he can and qualifies or modifies where he must; his work is solidly informed by the wealth of demographic scholarship, family and local studies, and archaeological findings of recent years. Not the least valuable aspect of the book is an interesting and appropriate collection of plates, figures, and sketch maps.

Some demurrers may be entered. The choice of dates, 1175-1425, is both fresh and intriguing; by that very token, however, there is a need to clarify and to justify the choice, but this is not clearly done. Perhaps too, the stress on unification is too great. In one sense it leads to too casual a use of premature terminology ("civil service," "gentry") and in a much more important sense fails to emphasize how overwhelmingly stratified and static society truly remained until the much later age of genuine urbanization and industrialization. Finally, there is the question of intended audience. The book clearly is too advanced for beginners, but it can be read with great profit by specialists, students, and scholar-teachers alike. The latter, certainly, will be

MICHAEL PRESTWICH. *The Three Edwards: War and State in England, 1272-1377*. New York: St. Martin's Press. 1980. Pp. 336. \$25.00.

In essence this book is a study of the English state and society during the period from 1272 to 1377, with particular attention being paid to the wars against the Welsh, Scots, and French. It has been designed, so the preface tells us, as a work of synthesis rather than as "the presentation of original research on a massive scale," yet one is always being pleasantly surprised by novel ways of interpretation as well as, particularly in regard to warfare and finance, by facts that are not readily to be found elsewhere. The text is a good blend of political, constitutional, economic, and social history; the church and intellectual activity have by design been largely omitted. The book is a very readable one; narrative or factual offerings are followed quickly by succinct yet lucid discussion, and the political chapters are interspersed with those giving a thematic treatment.

Michael Prestwich has a keen eye for memorable trivia, and the text abounds with apposite anecdotes and colorful examples. Some readers may think he overdoes it, for he finds it difficult to resist describing the nastier types of death, violence, and disease and giving the saltier pieces of contemporary gossip. Professional historians will probably wince here a little, but many other readers will, I think, find it all vastly entertaining.

As the title would suggest, the centerpieces of the book are the three King Edwards, and for this the author offers no apologies. Noting that it is not currently fashionable "to place kings in the centre of the historical canvas" (p. 294), Prestwich argues that the three in question deserve such treatment because their personalities had great influence on the course of English history. This is done convincingly not only in the chapters concerned with politics but also in those dealing with such themes as parliament, the nobility, warfare, and the fourteenth-century economy.

On the debit side there is the general lack of footnotes. Readers soon discover that notes have been used only to identify quotations and that, in the matter of tracing from where the large mass of supporting detail derives, they are on their own. There is a select bibliography, arranged by chapters, at the end of the book, but this is a poor substitute for footnote citations. The publishers would probably

argue that the book is not intended for the academic or the graduate student, but for the undergraduate. If this does not excuse them entirely, we must admit that concern for the undergraduate is well founded. In the last fifteen years or so there has been a great shortage of books that can be recommended to the university student who is starting a serious study of England in the later Middle Ages. Here at last is a good one.

J. G. BELLAMY
Carleton University

BERND SCHNEIDMÜLLER. *Karolingische Tradition und frühes französisches Königtum: Untersuchungen zur Herrschaftslegitimation der westfränkisch-französischen Monarchie im 10. Jahrhundert.* (Frankfurter Historische Abhandlungen, number 22.) Wiesbaden: Franz Steiner Verlag, 1979. Pp. vi, 241. DM 48.

How did the West Frankish kingdom become the kingdom of France? What role did memories of the Carolingians and their actual practices play in the process of transformation? These are the most important questions asked in this valuable and stimulating work. Bernd Schneidmüller claims that it is a study in political theory. He is too modest. The reader will also find a good deal of political history, some social history, and many comparisons with developments in contemporary Germany.

The book commences with a discussion of the deposition of Charles III and shows that the idea that the West Frankish kingdom was indivisible and that it could function outside a unitary empire emerged clearly at this time. Schneidmüller draws heavily on a famous article by Gerd Tellenbach ("Die Unteilbarkeit des Reiches," *Historische Zeitschrift*, 163 [1941]: 20–42), but he advances the argument considerably and focuses more on France, whereas Tellenbach was interested in Germany. The West Frankish kingdom was never again divided, and after 887 Carolingian traditions were evoked in various ways to transform West Francia into France. The most crucial element in this transformation was the gradual identification of kingship with the kingdom rather than with the king or with the Carolingian dynasty. The image and memory of Charlemagne were made distinctively French. Royal theology increasingly identified kingship as an office rather than as a personal dignity. Politically loaded geographical terminology appropriated the Carolingian imperial ideal and focused it squarely on Francia. Even non-Carolingian rulers, four of whom reigned from 887 to 987, sought to introduce themselves into the Carolingian tradition.

Having established this general theoretical framework, the author continues with two sets of specialized studies. First, he looks at monograms, artistic

representations, and architecture to show how Carolingian traditions were evoked and used. His point is to show how the Carolingian monarchy was kept alive in visibly symbolic ways. No fundamental changes were perceptible to contemporaries in coins, seals, diplomas, and the other instruments of rulership. Second, the "ruler-images" of each king from Odo to Robert II are studied, and the author concludes that for more than a century these kings remained remarkably true to a particularly Carolingian image of rulership.

That the tenth century saw a gradual change from Carolingian West Francia to Capetian France has long been known, and, therefore, in one sense, this book's conclusions are not very original. But, rooted firmly in a knowledge of political and social realities, based upon an impressive command of many kinds of sources, and marked throughout with the gift of asking imaginative questions, this book presents a clearer, fuller, and more persuasive discussion of the birth of Capetian France than has ever before been written. Moreover, the book will cause its readers to reflect on the tenth century as an age of positive, solid achievement and considerable creativity. We read in these pages of France being born, not of the Carolingian world dying. This shift of focus is subtle but important.

THOMAS F. X. NOBLE
University of Virginia

JOSEPH M. TYRRELL. *Louis XI.* (Twayne's World Leaders Series, number 82.) Boston: Twayne Publishers of G. K. Hall, 1980. Pp. 201. \$12.50.

The reign of Louis XI (1461–83) was an important time in French history, but it is very difficult to write about. The voluminous records of the royal chancery, parlement, and *cour des aides*, as well as the municipal archives of the period, are unedited and for the most part even lack thorough inventories. By contrast, the extensive narrative sources not only have been published but are dramatic and entertaining as well. They tell a good story and have long been mined by storytellers, but historians have become increasingly dubious of their reliability. When still a graduate student, this reviewer discussed Louis XI with the late Robert Fawtier, and I vividly recall Fawtier's remark that some promising young French scholars had set out to study Louis XI and had never been heard from again!

Joseph M. Tyrrell thus accepted a difficult chore in undertaking a 200-page book on Louis XI for the Twayne World Leaders Series. He has done a very creditable job in producing a reliable and up-to-date survey. There was, of course, no question of plunging into the uncharted wilderness of administrative manuscripts. Tyrrell's great accomplishment

has been to rewrite the history of Louis XI for the general reader in the light of the research of Karl Bittmann, who exposed the errors of the once highly regarded Philippe de Commynes, and Richard Vaughan, who has written the comprehensive recent study of Charles the Bold of Burgundy.

The result is a highly factual, tightly organized narrative that places the greatest emphasis on the famous struggle with Burgundy. The author periodically calls our attention to those points that differ from older interpretations. He points out, almost apologetically, that the story is not as exciting as it used to be, when Commynes was regarded as authoritative and the duke of Burgundy was more readily characterized by his French surname of "the Rash."

The book is a bit skimpy on the king's youth and his long and curious estrangement from his father, but Tyrrell gives us a lucid picture of the political and diplomatic aspects of Louis's actual reign. Besides the all-important Burgundian question and relations with other, less dangerous princes, Tyrrell considers French relations with Spain, Milan, the empire, and especially England. It is regrettable that he did not find room to say more about Franco-papal relations, which were of considerable importance under Louis XI. The scope of the work does not lend itself to a coverage of economic affairs, but one should note in passing that the French people endured an enormous increase in taxation during this reign without significant resistance from those who paid. The explanation must lie in the rapidly reviving prosperity of a badly ravaged kingdom. In this respect, as in others, Louis was lucky.

Thanks to Tyrrell's work, we now have a solid, up-to-date narrative of the main political developments in the reign of a competent king who was neither brilliant nor diabolical. For both the professional historian and the general reader, this marks the first step toward a genuine understanding of the period. Those who carry a spark of romanticism within them will doubtless mourn the passing of the "Universal Spider."

JOHN BELL HENNEMAN
University of Iowa

SUZANNE FONAY WEMPLE. *Atto of Vercelli: Church, State, and Christian Society in Tenth Century Italy*. (Temi e Testi, number 27.) Rome: Edizioni di Storia e Letteratura. 1979. Pp. viii, 246. L. 18,000.

None of the three tenth-century social critics, Liudprand of Cremona, Rather of Verona, and Atto of Vercelli, is easy to understand, and we must be grateful for a book that takes on the formidable task of reducing the life and thought of one of them to an intelligible pattern. This book, a dissertation de-

veloped by further manuscript research, widens particularly our knowledge of the period from about 930 to 960 and advances our understanding of such difficult works as Atto's *Polipticum*. Although perhaps overly ambitious, and therefore marred by an inadequate knowledge of some of the many topics taken up, the text supplies the reader with generous quotations from the sources, which can serve as a check on whatever is astray in the analysis. In six chapters and an epilogue, Suzanne Fonay Wemple treats Atto's literary method and sources, his political thought and social criticism, and his approach to church reform. She also compares his thought with that of Peter Damiani. Five appendixes consider Atto's testaments, the manuscripts and editions of his works, his literary and canonical sources, and his use of the Augustinian doctrine of grace and predestination.

Among the many things of value in the book is the continuing theme that Atto's ineffectiveness as a reformer sprang from his unwillingness to follow the Carolingian churchmen who used practical measures and the penitential system of the church to oppose royal interference in church affairs. Atto preached obedience even to tyrants. Wemple, following Cinzio Violante but arguing for an earlier dating, shows that what seems to be merely chaos in the mid-tenth century can be shown from Atto's works to be the emergence of new classes, occupations, and forms of life. Wemple's study of the canonical resources of Atto, particularly of the *Collectio Anselmo dedicata*, although admittedly not definitive, will be very useful to scholars.

Flaws in the book include the use of a large number of inferior editions, primarily of patristic writers. Here there is little consistency, for Wemple is often aware of and uses in parts of the book the best edition of a given work, only to cite a dated edition elsewhere. On a rather large number of topics the bibliography is capricious and again dated. What seems to be a failure fully to understand certain texts (such as that in note 72 on page 37) indicates, perhaps, the inadequacy of discussing biblical exegesis without reference to the work of Henri de Lubac. The frequent comparisons of Atto to Augustine tend to rest on a reading of Augustine in which the interpretations of scholars in the tradition of Adolf Harnack are prominent, while scholars like Etienne Gilson and Yves Congar are almost ignored.

GLENN W. OLSEN
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PETER RAYMOND PAZZAGLINI. *The Criminal Ban of the Sienese Commune, 1225-1310*. (Quaderni di "Studi Senesi," number 45.) Milan: Dott. A. Giuffrè Editore. 1979. Pp. 194. L. 6,000.

A common error committed by historians of the Italian city-state as well as by historians of law is to confuse the legal procedure of banning with the Roman institutions of exile and deportation. Unknown to Roman law, the *bannum* most likely derived from Germanic and especially Lombard customs and by the late thirteenth century had multiple and overlapping meanings. The ban signified public and formal notification of an impending event, obligation, or prohibition. Most importantly, it referred to the stages of the procedure in which the authorities of a community issued a peremptory command to someone to satisfy an obligation and to respond to a summons to appear in court or before a magistrate within a specified period. Disobedience of the ban normally resulted in penalties ranging from monetary fines to mutilation and to expulsion from the community. Confusion between the *bannum* and *exilium* has occurred because the most frequent mode of disobeying a ban was to flee the community in which it was issued. Indeed, communal authorities resorted to the ban not only to control civic comportment and enforce statutes against suspected and notorious criminals but also to expel from the community individuals, families, and even groups such as lepers deemed socially and politically dangerous.

Relying on legal tracts, statutes, and thousands of unpublished Sieneese archival records relating to the ban, thirty-three of which are published in this volume, Peter Raymond Pazzaglini presents a detailed description of the procedure of the criminal ban. He concludes that, although the proclaiming, enforcing, and abrogation of the ban were initially established by the *popolo* early in the thirteenth century, successive regimes supported, developed, and increasingly resorted to the ban in the prosecution of crime. To prosecute crime more effectively, according to Pazzaglini, Siena rationalized court procedures by eliminating traditional modes of proof such as ordeals and oath-helpers in favor of torture, witnesses, public denunciations, and notarial instruments. In addition, the Sieneese courts increased the severity of fines and the application of corporal penalties, imposed urban penal and procedural law upon the *contado*, and swiftly and vigorously punished "urban crime, whether the street violence of the feuding aristocracy or the nocturnal theft of the lower class immigrants from the *contado*" (pp. 100-01).

Although supported by copious, lengthy footnotes, Pazzaglini's conclusions would have been more convincing had he included a discussion of the ban in noncriminal cases and had he attempted to analyze statistically the more than twenty thousand extant records relating to the criminal ban in thirteenth-century Siena. His monograph complements William Bowsky's pioneering studies on

late medieval Siena, and it represents a competent and valuable addition to our knowledge of the legal procedures employed to intimidate and coerce the inhabitants of the late medieval Italian city-state.

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MICHEL BALARD. *La Romanie génoise (XII^e-début du XV^e siècle)*. In two volumes. (Bibliothèque des Écoles Françaises d'Athènes et de Rome, number 235, or Atti della Società Ligure di Storia Patria, new series, volume 28, number 1.) Rome: École Française de Rome or Società Ligure di Storia Patria, Genoa. 1978. Pp. 494; 501-1,008.

The rise of Genoese influence in the eastern Mediterranean during the thirteenth century depended on Genoa's diplomatic and military relationship with the hard-pressed Byzantine emperors and on Genoa's attempt to compete with the Venetians. Byzantine interest in the Genoese dated from the 1150s when Manuel I offered trade concessions in the hope that the Genoese would serve as a much needed counterweight to the Pisans and Venetians. Genoese influence, however, was insignificant before the Fourth Crusade and the subsequent Byzantine reconquest in 1261. During the rest of the thirteenth and fourteenth centuries the Genoese used their naval power in the Aegean and Black Seas to protect and expand their commercial interests in the East. By the mid-fourteenth century the Genoese supported various members of the imperial family in order to protect their interests. It was not a particularly good arrangement for the basileis, but Byzantine rulers rarely had viable alternatives.

Although political and diplomatic relationships provide the essential structure within which Genoese commerce flourished, Michel Balard concentrates most of his energies, and the most original parts of his book, on a social and economic study of the merchants and their trade. These merchants, as an anonymous poet noted, tended to found little Genoas wherever they went. Politically, economically, and spatially the patricians and later the *alberghi* dominated life in the eastern settlements just as they did in their metropolitan city. By a careful analysis of surviving notarial cartularies Balard shows that most of the officials and the most important traders came from Genoa itself. Settlers from the Ligurian coastal towns and inland villages were most often sailors and artisans. Easterners living in these little Genoas also tended to be isolated from the profitable international trade. The settlements seem to have been tolerant. Italians and Easterners shared living quarters, common names, and loan words; but there was little or no intermarriage or conversion between these groups.

The major turning point in the history of the *Romanie g noise* was the mid-fourteenth century. There was, of course, the Black Death—bringing decreased demand for grain and increased demand for slaves. But more significant was the shift of trade routes away from Asia Minor. The potential profits and commercial interest in light luxury goods declined after 1350. Henceforth the Genoese concentrated on trade in more profitable bulk items like alum and salt. Concomitant with the shift in trade, Balard notes the virtual disappearance of small investors, mostly artisans and recent immigrants, from the notarial contracts as commerce became “professionalized.” As successful as the Genoese were, they never managed to create a centralized colonial regime similar to the Venetian empire. There are indications that the settlement at Constantinople (Pera) was to have been a kind of colonial capital, but increased Turkish and Mongol pressure on various Genoese settlements made centralization impossible. The Genoese adjusted to local circumstances. Genoese weights, measures, and coinage never dominated.

Balard’s examination of the Genoese in the East is a well-crafted synthesis of previous works and his own original research, especially in the notarial cartularies. Compared to his contributions, the flaws are minor. His topical structure has forced him to plow the same ground more than once. Slaves, for example, are discussed once as inhabitants of the settlements and once again as objects of commerce. More seriously, although he recognizes that fourteenth-century Genoese merchants often preferred no longer extant account books to notarized contracts, he is not always able to adjust his series of data to allow for that change in business practice. Nonetheless, Balard’s reconstruction of Genoese commerce is a significant contribution to Italian and Mediterranean economic and social history.

DUANE J. OSHEIM
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MODERN EUROPE

T. S. WILLAN. *Elizabethan Manchester*. (Remains, Historical and Literary, Connected with the Palatine Counties of Lancaster and Chester, Third Series, number 27.) Manchester: Manchester University Press, for the Chetham Society. 1980. Pp. 163. £10.50.

At the beginning of the eighteenth century, Daniel Defoe described Manchester as “the greatest mere village in England.” It was technically a village because it had not yet been granted the status of an incorporated borough. Despite its size, it was still governed and administered by a Court Leet and the officers that that court appointed. In con-

sequence the historian is deprived of those voluminous corporation and company archives that facilitate the study of other towns in early modern England. Further difficulties arise from the fact that the parish, the manor, and the township of Manchester all had different boundaries, and hence their records must be used with considerable care. T. S. Willan is therefore to be congratulated on piecing together so much of Manchester’s Elizabethan history. He has used rentals, wills, and inventories to investigate the town’s topography, its trade and industry, and the economic activities of its two thousand inhabitants. Manchester was growing and prospering in this period, thanks to the demand for its linen and woolen products, and some of its merchants were amassing considerable wealth. This book provides a full picture of their trading contacts, their houses, and their possessions. It shows among other things the continuing importance of agricultural occupations, the uses of credit, and the definite improvement in living standards that came with increasing wealth.

Willan has not, however, attempted a complete history of the town as a social entity. He deliberately refrains from considering its religious life and its government, because the growth of Puritanism and the details of administration have been discussed by other authors. This is to be regretted, for it means that he gives us no rounded impression of the community of Manchester. There are signs that he doubts whether it was a community at all. He suggests, for example, that the one hundred officers appointed by the Court Leet were superfluous and inefficient. Yet they surely gave a large proportion of the inhabitants a stake in their town; they helped to bind it together and to create a sense of local identity. He also describes the conflict over enclosure that broke out between the lords of the manor—the Mosleys—and some of the inhabitants in 1603, but he does not go on to ask whether this reflected wider social tensions connected with the town’s economic growth and anomalous political structure. These are questions of general interest to historians of urban society, but Willan evidently dislikes such speculation. He sticks closely to the evidence, its details and its limitations. The result is a work that covers a substantial and important part of Elizabethan Manchester, but not the whole.

PAUL SLACK
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ANTONIA FRASER. *Royal Charles: Charles II and the Restoration*. New York: Alfred A. Knopf. 1979. Pp. xviii, 524. \$16.95.

Three centuries after his death, Charles II has lost none of his power to charm. Even Queen Victoria

named Charles II, for all his moral failings, as her most attractive predecessor. Antonia Fraser, the brilliant author of *Mary Queen of Scots*, *Oliver Cromwell*, and *James VI and I*, has also succumbed to Charles II's charm. She brings her tremendous powers of research, synthesis, and historical perception to this study, which she calls a "labor of love." The book, five years in the making, is in the nature of a reassessment, but not an apology. Fraser acknowledges her debt to all scholars of the period, past and present, and makes no claim to have gone beyond the printed sources and secondary works (for which some historians may criticize her). She offers important new interpretations on central questions of Charles's reign, his court, and his time. What emerges for the reader is not the legendary "Merry Monarch" but essentially a kind, generous, tolerant, witty, and yet a melancholy and serious monarch.

Fraser devotes the first third of her work to an analysis of Charles prior to 1660. His character was largely shaped by a series of harrowing experiences and challenges: the defeat and beheading of his father by Parliament; his family poverty, which obviously contributed to his practice of deception; his own helplessness in exile; his dramatic adventures after Worcester, where he kept not only his courage but also his head. As a result of these early experiences Charles developed a resiliency, a grit, and an ability to keep his own counsel.

When Charles II returned to England in 1660 he had very little power. When he died 25 years later he had almost absolute power and had triumphed over his enemies. How did this happen? On the surface it looks as though he survived with apparent ease, but in fact he utilized considerable political ingenuity and wile. Pragmatic rather than lazy, he considered it his main job to rule with the approval of Parliament. When it became obvious that he could not do so, he governed without it. He needed money, so he obtained that money from Louis XIV. "Charles had done with 500 kings. In future he would deal with one French king and rest master of his own fate." He also avoided confrontation whenever possible, the Clarendon affair being a case in point.

Fraser devotes a great deal of time and space to the subject of Charles II's religion. Two things can be stated with certainty: he was born a member of the Church of England and died 55 years later a Roman Catholic. The exact moment at which the change was made, first in his heart, and secondly in the form of the official conversion, cannot be determined with certainty. Fraser's analysis of the Popish Plot and the reasons for this mass hysteria is handled well. How could a country be reduced to such a state when the ratio of Catholics to non-Catholics was one to 100? Her answer is fear. Politics and religion were linked in the seventeenth century, and re-

ligion was not a private matter but a matter of state.

As Buckingham brightly observed, a king is supposed to be the father of his people, and Charles II was certainly the father of a good many of them. Fraser addresses the question of Charles's morals, maintaining that, despite his reputation for debauchery, the king behaved moderately for a royal personage. And if Charles set the moral tone for the court, he also had other interests characteristic of his age. He was genuinely interested in science, founding the Royal Society and Greenwich Observatory; he loved sports, particularly horseracing, gambling, swimming, and tennis; and he was a patron of the theater as well as the navy. He loved his dogs and was sociable, sane, and debonair.

Not all the questions raised by Fraser have been answered. Many readers will feel that, although some aspects of Charles II's life are described in minute detail, not enough is said on the question of French or Dutch diplomacy, his dealings with Louis XIV, the emerging empire, or local government. But this remains a very full treatment of an uncommon monarch in an uncommon age. It is not a definitive biography, for Fraser feels that finality is impossible—fortunately, in her opinion, for "who would wish the last word on King Charles II to have been spoken?" The thing that makes this excellent work perhaps the best of the lives of Charles II is the flow of the narrative. Fraser has pieced together the picture of a king and brought him to life for her reader. It is a work for both specialists in the period and the general reading public. Lord Halifax wrote about Charles after his death: "Let his royal ashes then lie soft upon him, and cover him from harsh and unkind censures." Because of this brilliant and charming book the ashes may well lie a bit softer.

RICHARD E. BOYER
University of Toledo

KAREN IVERSEN VAUGHN. *John Locke: Economist and Social Scientist*. Chicago: University of Chicago Press. 1980. Pp. xiv, 178. \$13.50.

In contrast with the history of political theory over the last fifteen years (and even with that of some branches of natural science), the history of economic thought is still for the most part written in a determinedly ahistorical and Whiggish vein as the internalist history of a discipline, from the viewpoint of that discipline's present. Karen Iversen Vaughn's study of Locke belongs firmly within this tradition, displaying some of its strengths but rather more of its limitations. In recent years attempts have in fact been made by several scholars—particularly Joyce Oldham Appleby, Keith Tribe, and

James Tully—to set Locke's conception of economic reality more firmly within the development of late seventeenth-century English society and of his own thinking as a whole. The last two of these scholars presumably published too late for Vaughn to benefit from their work, though it is a little surprising that she should neglect even to mention Appleby's 1976 article in *Past and Present*. (A more striking omission is the failure to cite—let alone to benefit from—K. H. D. Haley's massively informative biography of Locke's patron, the first earl of Shaftesbury, whose responsibility for Locke's interest in economic matters she correctly regards as decisive.)

Vaughn's handling of the broader historical context is in fact strikingly erratic, being full not merely of vagaries of judgment but of more or less trivial errors of fact. (The most remarkable single item perhaps is her apparent conflation of the Exclusion Crisis with Monmouth's rebellion.) Once she begins to assess Locke's economic analysis, however, the treatment improves sharply, and there are useful accounts of Locke's quantity theory of the market value of money, of the place of the ideas of vent and mechanical proportion within his general theory of price determination, and of his conception of the relations between economic entitlements, justice, and the working of the market. At various points she is able to offer valuable qualifications or corrections of the judgments of earlier scholars.

In conclusion she attempts to draw together the threads of her analysis by presenting Locke as "an early social scientist." This is no doubt intended as a compliment. But, both historically and theoretically, it is poorly devised to serve as such. The concept of "value-free" social scientist that Vaughn chooses to employ is too weakly characterized and too incoherently applied to be at all illuminating in itself. It is very feebly related to any understanding of Locke's developing theoretical views in either ethics or epistemology; and it fails to distinguish clearly between a general conception of the predictability of human performances, specific beliefs about economic causality, and assessments of rational conduct. If we are to succeed in complimenting Locke, we would be well advised first to take the precaution of understanding *his* view of what he was attempting intellectually to do.

JOHN DUNN
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EDWARD GREGG. *Queen Anne*. Boston: Routledge and Kegan Paul. 1980. Pp. xii, 483. \$45.00.

With the appearance of this volume, a generation of revision in Queen Anne studies comes to fruition.

Edward Gregg's work draws extensively upon recent scholarship, especially the publications of Geoffrey Holmes and Henry Snyder, and his analysis is buttressed by his own familiarity with the Continental archives. The result is a recasting of the traditional picture of the queen, which G. M. Trevelyan only retouched in his *England under Queen Anne*.

The queen's principal features, in Gregg's portrayal, are the seriousness (and the self-confidence) with which she exercised her responsibilities in state and church, the repugnance she exhibited toward the party leaders who bid recurrently for predominance in her ministries, and the loneliness and increasing isolation she endured as the Cockpit circle of earlier days was broken, especially by her quarrels with Sarah, Duchess of Marlborough and then by the death of her consort, Prince George. To be sure, the long-standing view of Anne as an emotional, dependent personality, and a stubborn adherent of the Church of England, is not wholly unfounded. Even Gregg does not deny the queen's deep-seated abhorrence of any Hanoverian presence in England while she lived; instead, he draws an analogy to Elizabeth I's seemingly similar conduct. Yet, perhaps he does discount the political costs both to her and to her ministers that maintenance of this stance, especially during her last years, entailed; in any case, the conciliar factions of the 1590s cannot be compared easily to the political parties of the early eighteenth century. Nonetheless, Gregg can show that Anne's initial support for the war against France, her backing of the Anglo-Scottish union, and her subsequent determination to bring the War of the Spanish Succession to a profitable conclusion for England were critical in shaping the triumphs of the reign.

Although Gregg's account of Anne's earlier years lacks depth and is marred by minor errors, once he arrives at 1702 he paints a more persuasive portrait. Thus, we now have a scholarly biography of the queen that can safely be shelved between Stephen Baxter's *William III* and Ragnhild Hatton's *George I*.

HENRY HORWITZ
University of Iowa

BRUCE LENMAN. *The Jacobite Risings in Britain, 1689–1746*. London: Eyre Methuen. 1980. Pp. 320. £12.00.

Bruce Lenman has undertaken in this book a greater task than its title indicates. He has been concerned, he tells us on page 9, with "the factors and motives which made men active Jacobites, rather than with a detailed narrative of events which have often been narrated before." Those fac-

tors and motives were economic and social as well as political, and they created discontent with post-revolutionary government in Great Britain. The actual risings of 1689, 1715, 1719, and 1745 receive less than full treatment here. Indeed, Lenman exposes to view grievances that must have been very important to men and women of the day and shows that the government's policies in economic and social matters, especially its decisions affecting Scotland, alienated some of the political classes. It is a pleasure to see a professional historian of Scotland at work bringing to bear many of the sources for the history of the period: printed sources, ephemera, and secondary works, with added material from the General Register House at Edinburgh, the British Library, the Public Record Office, and the Royal Archives at Windsor, to name only some of the more important places.

The organization of the book is idiosyncratic, with some repetition and enough "flashbacks" to furnish an arty film, sometimes clumsily introduced. Signs of haste are also evident, such as clichés, excessive slang, Scottish words not often used in English, illogical use of "but," and unidiomatic expressions that greater care in writing would have eliminated. On page 204, Lenman gives the Old Pretender's age as thirty in 1730. The author is at his best when dealing with Scotland's society and economy. He rarely mentions a figure without saying something of him (or her) to help the reader understand that person's actions.

Unfortunately, Lenman dislikes vigorously all the British monarchs from William III to George II and most of their ministers. He also detests James II and scorns most of the exiled Jacobites, who were not really as stupid and vicious as he seems to think. He allows his feelings to affect his tone, and that for the worse. And is it not naive to expect foreign governments, such as that of France, not to sacrifice the safety of Jacobites for a useful military diversion?

Thus we must always take account of Lenman's slant while using his book. That is a pity, for there is much to be learned from it.

GEORGE HILTON JONES
Eastern Illinois University

HILLEL SCHWARTZ. *The French Prophets: The History of a Millenarian Group in Eighteenth-Century England*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xvi, 382. \$25.00.

The French Prophets—by no means all French or all prophets—have fared badly at the hands of historians. The movement was little concerned with the political and social issues that have brought earlier millenarian groups to the historian's notice. Its preoccupation with prophecy, miracles, and even

the raising of the dead has not won it much attention from students of eighteenth-century ideas. Hillel Schwartz has filled the gap with a sympathetic, thorough, and massively documented account. Although the movement was always small—the author traces only about five hundred followers over several decades—it provides an excellent case study of the evolution of ideas and behavior in changing circumstances.

The original prophets, arriving in London in 1706 as refugees from the Camisard revolt, were molded by their upbringing in the "desert" of post-revocation France. Their visions of apocalyptic cataclysm grew naturally from their experience of persecution, apostasy, and bloodshed. The leaders of the Huguenot community already in London, with a very different experience since 1685, were suspicious and then hostile. Most converts were found among the English: Anglicans, Dissenters, and an important group of Philadelphians. All were inspired by the spiritual dynamism of the prophets. The converts, however, belonged to very different religious traditions and to a social milieu very unlike that of the Cévennes. Inevitably, as the English prophets came to the forefront, the character of the movement underwent drastic change. In Schwartz's terminology, the ethos of cataclysm evolved into anticipation of a broad, tolerant New Jerusalem and sometimes into a stress on inward spiritual gifts, the "pentecostal ethos." With only a loose organizational structure and an undefined doctrine on the nature of the approaching apocalypse, the movement was always fluid and was susceptible to extraneous influences of all kinds. Born with the fiery apocalyptic mentality of the seventeenth-century sects, the prophets came to share the attributes of Pietism and Quietism. Female prophets, always important, took a dominant role in the later phases of the movement.

Schwartz's book follows the group until its disappearance in the 1740s and explores the personal and social basis of its appeal (primarily to the substantial urban middle classes). More prominent followers included Sir Isaac Newton's friend Nicolas Fatio, F.R.S., who suffered for his faith on the scaffold where he was pelted with filth by the London mob. Schwartz insists that the prophets were not merely eccentrics. He successfully places them in the broader intellectual milieu and relates their outlook and appeal to the latitudinarian-scientific-millenarian pattern of thought that is now recognized as a major feature of the late Stuart period.

BERNARD CAPP
University of Warwick

LESLIE MITCHELL. *Holland House*. London: Duckworth. 1980. Pp. 320. £18.00.

"Full of crankiness, history and good sense, it was one of the most exciting places in London," is Leslie Mitchell's conclusion on Holland House, the great Whig *salon* presided over by Lord and Lady Holland for the first four decades of the nineteenth century. Especially as regards their view of domestic politics, there is a good deal more in this book about crankiness and history than there is about good sense. Nor is it really about Holland House. A *salon* thrives on conversation that is largely irretrievable. One is reduced to reliance on letters and journals, and because Lady Holland had more than her share of crankiness and was rather short on common sense, Mitchell is doubtless right to rely mainly on those of her husband. The reader therefore gets a very large dose of Lord Holland, more perhaps than is conducive to excitement.

But if this book does not amuse, it undoubtedly instructs. Holland never held an office of major influence, but Mitchell is quite right that because of who and what he was, he was one of the most important forces in Whiggery. As Fox's nephew, with a large measure of his uncle's passion and genius for friendship, he was the conscience of the Whig party. Contemporaries such as Lord Grey were never allowed to forget from whence they had sprung, and younger men, most notably Lord John Russell, were brought up by Holland on the pure milk of Foxite Whiggery.

Whether this gave rise to a party excessively dominated by its own history is rather more questionable. It is undoubtedly correct, and an important point, that Holland, and Grey too, early formed the views that they would carry with them, very little changed, throughout their subsequent careers and that their lives would be dominated by the great issues to which Fox had given his blessing in the 1790s: parliamentary reform, Catholic Emancipation, repeal of the Test and Corporation Acts, and the abolition of the slave trade and slavery. But it was scarcely their fault that it took so long to carry these questions, and they were hardly marginal questions.

Mitchell's list of secondary sources is a short one. In itself there is nothing wrong with this. But when we are told that Thomas Clarkson was an "American" abolitionist (p. 94), that "in the final struggle for Catholic Emancipation in 1829, the Dissenters behaved splendidly" (p. 109), and that Lord Lansdowne was leader of the House of Lords in Canning's government (p. 140), we are perhaps justified in concluding that it might have been longer.

It may be that Mitchell's neglect of secondary sources sprang in part from a desire not to color his reading of the primary sources. And his decision to immerse himself so completely in Holland is certainly not without merit. The result, sometimes in spite of the author, is to give us a much fuller un-

derstanding of Whig positions on domestic questions. And rarely if ever have the tortuous Foxite views on foreign affairs been better elucidated. For this Mitchell deserves great credit.

RICHARD W. DAVIS
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SHEILA FLETCHER. *Feminists and Bureaucrats: A Study in the Development of Girls' Education in the Nineteenth Century*. New York: Cambridge University Press. 1980. Pp. viii, 249. \$29.50.

The lure of endowments tantalized the English educational reformer. It was fitting that feminists, too, became distracted by the chimera. Their cause took added force from the fact that all-male endowed schools often could not justify their sexual bias with the title deed. After a half-century of reform, the Endowed Schools Act of 1869 made potentially sweeping changes. An ambiguous section of that act allowed application of endowed funds for girls' education, and the commission created to enforce the act was not shy about reconstructing endowments. In the term of the commission (to 1874) some 300 endowments were changed, or about 10 percent of those eligible. In the process, the commissioners generated hostility, and the control of endowments was returned to the charity commissioners. In this period, some 47 schools for girls were established and provisions for girls' education were made at 30 others.

Several directions are possible in interpreting this brief episode, for it joins the feminist movement and the reform of administration, education, and charitable trusts. Sheila Fletcher deals with all of these, and her method is to weave them together. The compound poses some difficulty, for no one theme dominates, and finally the reader is not sure what the rendezvous of feminists and bureaucrats meant.

A more serious weakness is in the placement of the subject in its historical setting. By not developing the events that preceded the Taunton Commission, Fletcher implies an unwonted novelty. Her commissioners were part of a fairly long line of "bureaucrats." As to feminism, and especially the issue in question, we want to know the dimensions of girls' education prior to the act of 1869. Finally, when explaining the "dead hand" of trust donors, Fletcher listens too readily to her heroes and heroines. Trust deeds had been an object of inquiry and reform—not, to be sure, feminist reform—for nearly a century before the Endowed Schools Act.

The feminists and the bureaucrats both belonged to a progressive social order. They both believed in an active and renovative government. Given the zeal of the commissioners, the record of reform was

truly disappointing; given the conditions of endowed education, the record was much more impressive. Yet endowed funds were always a limited resource. The campaign probably had its greatest success in secondary areas: prompting new foundations and promoting the general recognition of girls' education as a social necessity. One wishes that Fletcher had chosen to integrate her interesting story with this larger saga.

RICHARD S. TOMPSON
University of Utah

BERNARD PORTER. *The Refugee Question in Mid-Victorian Politics*. New York: Cambridge University Press. 1979. Pp. 242. \$34.50.

Compared with the United States, Great Britain has never been a land of mass immigration. Between the arrival of the Huguenots after 1685 and that of the East European Jews after 1875, it was politics, not poverty, that drove people to its shores. Italians, Germans, Poles, Hungarians, the remnants of every failed revolution between 1815 and 1848, found a home there, at least temporarily. Today, the government may grant political asylum at its discretion; during the last century, it could not have refused to do so—and not merely because there was no effective alien law. Indeed, on the single occasion—the Orsini affair of 1858—when a British government attempted to modify the traditional policy of asylum to a relatively small degree, it was unceremoniously voted out of office. To be sure, as Bernard Porter tells us, much more was at stake than the refugees themselves or the relatively recent tradition that had made Britain their haven from political reprisals. But it was no coincidence that it was upon this issue that Palmerston's enemies were able to muster sufficient support to separate him from his political base and to force his resignation.

The "right of asylum" was not in itself as significant as the whole complex of attitudes that it encapsulated and whose working out is the most interesting and important element in Porter's work. Through an analysis, in turn, of the points of view of the refugees, the Continental governments, the British public, and the British governments, he demonstrates that the right of asylum was rooted in a xenophobia that was, paradoxically, liberal. It rested upon the assumption that liberalism, like capitalism, was an expansive, dynamic creed, that its progress was inevitable, and that Britain's form of government, with its liberal political institutions and traditions, was its highest expression. If Continental despots chose to repress the just and moderate political aspirations of their peoples, they had only themselves to blame for the explosive consequences. Moreover, if they attempted to make

high-handed demands of Britain for the prosecution or extradition of those of their own liberals who had managed to escape their secret police, they had to be resisted, not merely for the sake of the refugees but in the sacred name of every liberal tenet—freedom of speech, of assembly, of conscience—that the Victorians held dear. And, not least, the assumption that peaceful compromise would be reached after the widest-ranging debate possible—which was the genius of the British constitution—demanded that the refugees be allowed the same liberties as Englishmen.

This is a well-written and thoroughly documented monograph. In the end, Porter does not answer the question he himself poses—namely, why the government did not pass an alien law until 1905, although the material foundation of liberalism had long since begun to break apart. That question, although beyond the scope of his book, indicates the value and the place of Porter's work in the continuing debate on the nature and evolution of Victorian liberalism.

BERNARD GAINER
University of Kansas

ALAN SYKES. *Tariff Reform in British Politics, 1903–1913*. New York: Clarendon Press of Oxford University Press. 1979. Pp. ix, 352. \$43.50.

Joseph Chamberlain's tariff reform campaign has long attracted historians fascinated by the apocalyptic and confusing nature of the mission. This book presents a very coherent and persuasive ideological explanation of the movement, supported by the most thorough reconstruction yet written of the high politics of tariff reform.

Alan Sykes sees Chamberlain as having raised questions of property, taxation, and collectivist legislation that were more fundamental to British political developments than Irish Home Rule. Chamberlain's platform in 1903 returned political debate from Ireland to the reform controversies that had threatened to overturn traditional political alignments before 1886. For Chamberlain and his Radical Unionist followers, all aspects of the tariff reform case were interrelated. Tariffs were presented as the means to increased prosperity and productivity, the difference between world power and impotence. By claiming to offer concrete economic gains coupled with sacrifices for the empire, tariff reform was to be the gospel by which labor would be attached to the Unionist party and class politics averted. Seen in this comprehensive light, tariff reform was a rival ideology to the new Liberalism in its approach to economics and society. Like its rival, it was based on underconsumptionist and collec-

tivist assumptions. Both ideologies rejected, more or less, the assumptions of laissez-faire free traders, whether Unionist or Liberal. From Sykes's account, Chamberlain can be ranked with Lloyd George as one of the two most daring and imaginative politicians of the era.

But Radical Unionism failed, and by 1913 tariff reform had been transformed into a policy to defend traditional Conservative values and institutions. It was this transformation, Sykes maintains, that dictated that by 1913 the Liberals would be the party of change and the Unionists "the party of resistance in social as well as constitutional questions" (p. 6).

The reasons for Chamberlain's failure are now clearer than ever. There were the Unionist free traders who fought him more bitterly and successfully than any similar group ever resisted Lloyd George. Balfour's skill, particularly after 1907, enabled him steadily to direct a radical policy toward conservative goals; to shift from an imperial policy to one largely domestic. The most important causes of failure lay in the nature of the Unionist party and in the tariff reform message itself. By 1903 most Unionist politicians and voters were too encapsulated in middle-class values to redefine their roles and attempt to absorb the growing labor movement. Even more basically, tariff reformers, despite strains of radicalism, never envisaged significant changes in the social order, especially in the relationships among classes and the distribution of political power. The great miners' strike of 1912 revealed dramatically just how hostile and blind even Radical Unionists were to the aspirations of labor.

The book has flaws, for some of the theoretical statements are undeveloped or even ambiguous. Some chapters concentrate on overly detailed political maneuverings and neglect to identify minor figures who appear. But all historians of British Conservatism will find this book essential, as will future biographers of all the leading Unionists.

RICHARD A. REMPEL
McMaster University

JOHN TURNER. *Lloyd George's Secretariat*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1980. Pp. vii, 254. \$27.50.

One of the major reasons there is such a vital and productive Lloyd George industry among historians of modern Britain is that students of his premiership—indeed, of his entire career—are fascinated by his disregard for established practice, for convention and custom. During the First World War he created the Ministry of Munitions, the War Cabinet, and the 1916–22 coalition itself. Each was a great success; each has been written about by

scholars of the period who have helped us to understand the Welsh Wizard for what he was: the first modern British premier. John Turner has done a similar important service, for he has examined the creation and working life of one of the most mythologized of Lloyd George's handiworks, the infamous "Garden Suburb."

Taking its curious name from the fact that its first home was in temporary buildings on the grounds of the prime minister's official residence, Lloyd George's secretariat was actually a special liaison between the prime minister and various cabinet departments or other organs of the state. At times it functioned as an unofficial information-gathering network or as a captive brains trust to report innovative solutions to knotty problems to the harried prime minister. Turner's clear explanation of the unorthodox scope of the body should forever end confusion of it with the more official War Cabinet Secretariat presided over by Colonel Hankey.

The story told in this book is that of the interaction of a number of crucial problems—Ireland, India, "drink," and agriculture—with the remarkable inmates of the "Suburb." They were W. G. S. Adams, the Gladstone Professor at Oxford; Philip Kerr, a former Milner Kindergartener whose twin passions were the empire and Christian Science; Waldorf Astor, the Anglo-American proprietor of the *Observer* and noted temperance crank; David Davies, a wealthy coal magnate and social reform enthusiast; and Joseph Davies, a self-made man and the first modern commercial statistician. Somewhat later was added Cecil Harmsworth, third of the five famous brothers and a Liberal M.P.

The "Suburb" was not, of course, an unqualified success, and it was the target of much criticism in the interwar years. It was created by Lloyd George to increase his control over the growing machinery of the state. Underfunded, undermanned, and underpraised though this secretariat was, British premiers in our time have returned to a similar answer and, in a way, vindicated both Lloyd George and the "Suburb." This excellent book has at last pointed that out to us.

R. J. Q. ADAMS
Texas A&M University

KENNETH MORGAN and JANE MORGAN. *Portrait of a Progressive: The Political Career of Christopher, Viscount Addison*. New York: Clarendon Press of Oxford University Press. 1980. Pp. ix, 326. \$42.00.

Christopher Addison is one of two senior Coalition Liberals whose political careers Lloyd George failed to ruin. The other of course was Winston Churchill. Through most of the First World War and until his dismissal in 1921, Addison was Lloyd George's viceroy in charge of domestic radical politics. He pro-

vided visible evidence as well that the prime minister remained true to the principles of reform. After his fall, Addison was able to build a second career as a Labour minister and served as the party's leader in the House of Lords during the energetic years of the Attlee administration. With an active political life of over forty years and as a member of the cabinet in three separate governments, he has long deserved a better study than the appalling hack biography produced by R. J. Minney in 1958.

The present book began as Jane Morgan's doctoral dissertation under R. H. Evans at the University of Leicester and possibly is evidence in support of the conventional assertion that aspiring Ph.D.s should not undertake full-scale biography. Nevertheless, there is much useful material here. Addison's later career as a Labourite—much the best part of the book—is fully treated. Too many historians have dealt with Addison only in terms of his unhappy service at the Ministry of Health, ignoring his highly successful tenure as Minister of Agriculture in MacDonald's second government and as the genial elder statesman who put Labour socialism through the House of Lords after the Second World War.

Yet there are elements in the book that make it less than fully satisfactory. The authors—although it may be assumed that the study under review is chiefly the work of Jane Morgan—somehow deal with Addison at a distance. The letters quoted tend to be those he received, not the ones he sent. There is too much general history followed simply by the assertion that Addison was involved. One knows that he was responsible for legislation creating, for example, the Ministry of Health or the Marketing Boards, but one never finds out how he put the bills together nor, indeed, much about the administrative structures themselves. There is, in this connection, a rather bad error on pages 95 and 96 in the statement that the Local Government Board continued to exist after the creation of the Ministry of Health. The Ministry of Health absorbed, or in fact was, the Local Government Board renamed, with the addition of the Health Insurance Commission and certain other health powers from the Home Office, Privy Council, and the Board of Education. Local government supervision and the much-despised Poor Law Division remained intact within the new ministry.

Nonetheless, as the life of an important, but almost unknown, figure, the Addison biography makes a useful contribution. It is no criticism to argue that there is more to be said about him.

BENTLEY B. GILBERT
University of Illinois,
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URI BIALER. *The Shadow of the Bomber: The Fear of Air Attack and British Politics, 1932-1939*. (Royal Histori-

cal Society Studies in History, number 18.) London: The Society. 1980. Pp. 166. \$21.00.

One of the ironies of World War II is that Britain's air force, which played such a crucial role in its outcome, was built on the basis of a miscalculation. That miscalculation gave rise to a fear of the absolute destructiveness of the bomber, which, Uri Bialer argues, heavily influenced not only Britain's rearmament policy but also its foreign policy in the thirties. While many scholars have commented on the origins of the fear of the bomber and different aspects of its effect on British policy in the interwar period, Bialer has pulled all the strands of the story together.

Drawing on a wide range of secondary source material, he develops the tale from the origins of the fear in the very limited German bombing of London at the end of World War I. The effect of that experience on the national psyche, combined with some outlandish statistical extrapolations of the havoc that would be wreaked if bombing were conducted on a more extensive scale, persuaded many Britons that, in the next war, the nation could be "knocked out" by a blow from the air.

Prominent among those who believed in that possibility were many of the leaders of the national government in the thirties. Stanley Baldwin gave unambiguous expression to the government's concern in a 1932 speech to Parliament, warning the nation that "the bomber will always get through." While not as publicly outspoken in their concern, Neville Chamberlain, John Simon, and Samuel Hoare were all deeply worried about the threat posed by bombing. This concern came into sharp focus in the mid-thirties, when it became apparent that Germany was bent on building a powerful air force.

The story of Britain's rearmament in the thirties, with its early emphasis on developing a bomber force that would be a deterrent to Germany, has been analyzed by many scholars, including G. C. Peden, Stephen Roskill, and myself. Bialer notes that only the Admiralty and the War Office challenged the doctrine of deterrence, arguing that the technology had not yet been developed to enable Germany (or Britain) to deliver a knockout blow from the air, and pointing out that all available evidence indicated that the air force Germany was developing was being designed and deployed to provide the army with ground support. The truth of these arguments to the contrary, the national government continued to direct its efforts toward protecting Britain from bomber attack.

Bialer discusses at length Britain's successive efforts to achieve international disarmament in the air, collective security arrangements to deter air attacks, and rules limiting aerial warfare. He argues that while each of these efforts ultimately failed—

and Britain was busy rearming to the hilt even while conducting them—the record of internal debate indicates that they were undertaken sincerely by the government and are a dramatic illustration of the lengths to which Britain was willing to go to minimize the potential threat of air attack.

The Shadow of the Bomber is a model monograph, in which Bialer judiciously stitches together secondary and primary source material and draws the fabric tightly around his subject. His footnotes, which are well conceived, are a good guide to the present historical literature on British politics in the thirties.

My quibbles about Bialer's book are three. In my reading of the government papers I did not feel the sense of urgency after 1934 for international limitations on air attack that he contends was present through 1939. Second, while he discusses with admirable clarity the factors that contributed to the fear of the bomber and the development of the policy of deterrence, he conveys little sense of the relative importance of those factors. Third, it is a shame that there should be so many typographical errors in such a thoroughly researched, well-written, and otherwise elegantly produced little book.

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MYRNA CHASE. *Elie Halévy: An Intellectual Biography*. New York: Columbia University Press. 1980. Pp. x, 293. \$17.50.

Anglophilia was a recurrent condition of many intellectuals in nineteenth-century France. Guizot, Tocqueville, Taine, and a host of lesser literati marveled at what Myrna Chase calls the "English miracle": economic modernization, social reform, and political liberty, all without revolution—what an enviable achievement from the perspective of a nation plagued by all manner of ideological divisions and social dislocations! The historian Elie Halévy resumed in the twentieth century this tradition of Gallic admiration for English institutions. He appears in Chase's account as the quintessential liberal of the Third Republic: fearful of revolution, suspicious of government power, and intent on preserving the humane values of individual freedom against encroachments by state, church, party, or people. Accordingly, he became a misanthrope in his own turbulent society, forever regretting the absence of that tradition of measured liberty and orderly reform that he celebrated in his studies of English history.

Halévy's sympathetic portrayal of English society has undergone extensive criticism from historians of the left, and Chase has felt impelled to leap to his defense. She dismisses the assaults of E. P. Thompson and Eric Hobsbawm as mere nitpicking by ungrateful subalterns who should have the grace to

acknowledge the master. Hobsbawm's denunciation of Halévy's liberal prejudices "patently fails to appreciate that his political values led him to pose questions that give his work its transcendent (*sic*) quality, a quality transcendent (*sic*) enough to still shape the work of men like Hobsbawm, Thompson, and their disciples" (p. 91). But Chase sensibly recognizes the ideological basis for this historiographical dispute. Whereas the English neo-Marxists saw in evangelical religion and oligarchical government the agents of repression and cooptation in nineteenth-century England, the French liberal historian hailed them as socially beneficial mechanisms for defusing revolutionary discontent and promoting class harmony.

His lifelong preoccupation with the twin themes of political liberty and social stability rendered Halévy implacably hostile to socialism. While he is best known in the English-speaking world for his studies of English society, he is remembered in France for his critical evaluation of European socialism. Marxism was repellent to him because it rested on the materialist conception of history, Fabianism because it promised social regimentation and the bureaucratization of politics. Even the tame socialism of Jaurès was suspect because of its anti-militarist overtones. In his later years he went so far as to hold the socialist tradition responsible for the aggrandizement of state power during the First World War and the rise of fascism thereafter.

Halévy wrote history from the conviction that ideas constitute the motive force in human affairs. This is a conviction evidently shared by his biographer. What Chase represents as an "intellectual biography" is in reality an extended analysis of Halévy's thought. After a brilliant opening chapter treating the familial and educational influences on the young historian as well as his political involvements during the Dreyfus affair, Chase's work ascends to the ethereal realm of pure intellectual history. It concludes with a twenty-seven-page chapter summarizing the positions taken by Halévy and various interlocutors at a single debate in November 1936. The reader finally puts the book down with a feeling of having acquired a profound understanding of the historian's ideas mingled with a sense of disappointment at having learned very little about the man.

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A. W. WRIGHT. *G. D. H. Cole and Socialist Democracy*. Oxford: Clarendon Press of Oxford University Press. 1979. Pp. 301. \$36.00.

The late G. D. H. Cole was a sort of human word-processor. In the fifty years from 1910 to 1960 (and

excluding the numerous detective thrillers he wrote jointly with his wife Margaret) he wrote or edited no fewer than 99 books, and literally hundreds of articles, reports, and review essays. He was an indefatigable lecturer who, along with his equally prolific contemporary, Harold Laski, represented to an entire generation of left-wing intellectuals the very epitome of the academic *engagé*. So much brainwork; so prodigious an intellectual product. And yet, as many have been tempted to conclude of Cole's (and of Laski's) legacy, so little wisdom. "A Bolshevik soul in a Fabian muzzle," quipped Maurice Reckitt, early pronouncing Cole "a bit of a puzzle." Clement Attlee said of him, somewhat later, that he was "a permanent undergraduate." And more recently, Ernest Gellner has characterized his voluminous writings as "an unselective, eclectic, unfastidious mishmash."

But Cole has his admirers as well as his critics. Noting at the outset of the book under review "a rediscovery of Cole and his concerns," A. W. Wright adds his generally approving voice to those of several other authors who have devoted considerable energy over the past decade to a sympathetic rehearsal of the case for Cole. What is the source of this renewal of interest in a writer whose reputation seemed until recently to be in eclipse? It is the apparent belief among elements of a new generation of British socialist intellectuals (whose political outlook may be broadly described as that of the New Left) that Cole's theoretical interests constitute at key points a starting place for their own. In his search for a radical theory of socialist democracy and in his vision of a new decentralized society of Rousseau-like fellowship and participation, Cole represents to these admirers a prophet unheeded in his own time, a moralist whose long-ignored teachings offer hope for recovery from the theoretical bankruptcy (as they see it) of the labor movement in twentieth-century British politics.

Given Cole's daunting prolixity, Wright has his work cut out for him in attempting a review of the entire corpus of his work. The problem is compounded, as the author acknowledges, by Cole's constant urge "to be relevant, even at the cost of much wearisome toil, and involving at least the partial sacrifice of intellectual nicety." Wright's view is that we can best account for the frequent ambiguities and admitted contradictions in Cole's myriad theoretical pronouncements by assuming that "a leading and distinctive feature of his thought" was a tendency to operate at two levels, those of "practical relevancy" and of "a wider socialist position." Wright terms this tendency "dualism" or "bifocalism," and his exegetical industry in attempting to apply it to Cole's writings is never in doubt. Readers will have to judge for themselves the extent to which he succeeds in establishing the plausibility of this approach. For this reviewer, at

least, the case is never entirely satisfactorily made. There remains a lingering sense that while Cole was indeed a man "literally full of ideas," he was usually so busy scribbling them down and publishing them that he had precious little time left over to think, and that consequently, the "ambiguities" and "contradictions" we encounter in reading him amount to nothing more and nothing less than these terms signify in common usage.

The book is divided into two parts, in the first of which the author provides a background account of the development of Cole's guild socialism. Inspired by admiration of William Morris, Cole's vision of a new socialist order rooted in active fellowship and freed of the trammels of the central state, argues Wright, lay at the heart of a lifelong "wider socialist position." In the second and longer part of his book, Wright turns to an examination of Cole's major concerns of "practical relevancy," including his situational responses to the political events of his day, his economic views, his assessment of Marxism, and his attitudes to developments in Russia and in the sphere of socialist internationalism. Here we learn that, in politics, Cole (who once called himself, with characteristic ambiguity, "a sensible extremist") was a champion of "constitutional revolution," in economics, both capitalist reformer and socialist dreamer; that he made Marx out to be a democratic socialist not unlike himself and combined fuzzy criticisms of Stalin with general support for the Russian regime; and that he was both Little Englander and socialist internationalist.

Wright has been at great pains to make sense of a particularly frustrating thinker. He calls on his reader to see Cole as a complex man who deserves to be understood as "a democrat of a special kind." Yet the picture that emerges is of a quintessentially muddled political intellectual who wanted to have his cake and eat it too. Wright, to be certain, is not uncritical of his subject. But there is perhaps too much of the apologist in a point of view that asks us to accept Cole's "loose framework of economic determinism," his "loose historical materialism," his "loose embrace of Marxism," his "accommodative, eclectic style," and the "looseness of Cole's theoretical analysis" as the price of sympathetically understanding his "dualism" and "bifocalism." It hardly takes a specialist in modern political philosophy to think of substituting for these esoteric terms such down-to-earth words as "confusion" and "obfuscation."

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STUART MACINTYRE. *A Proletarian Science: Marxism in Britain, 1917-1933*. New York: Cambridge University Press. 1980. Pp. xii, 286. \$27.50.

When Stuart Macintyre tries to sum up his book, he says that the main argument is that British understandings of Marxism changed from the positions taken by a variety of autodidacts, largely working class, to more scholarly and professional versions that were increasingly shaped by Marxism-Leninism. He is apologetic about the historical significance of this argument, with good reason: he cannot claim that these theorists were major thinkers or that the changes in understandings of Marxism made it more influential or more successful in dealing with British politics. He also realizes that the straightforward intellectual historical approach implied by this main argument would be a peculiarly un-Marxist way to approach Marxism.

In fact, Macintyre has written a book that is much more complicated and interesting than his main argument would imply. He has tried hard to place British Marxists in the perspectives of the evolution of British industry, the British working classes, and British socialist ideas. He discusses the industries and localities in which a Marxist consciousness most commonly evolved, and he has interesting things to say about the impact of a centralized British Communist party on British Marxism. The largest part of the book describes the positions reached by British Marxists on major Marxist doctrines (for example, historical materialism, the dialectic, the state, class and class consciousness) and contrasts these positions with the Labour orthodoxy of MacDonald and Snowden and the Marx-influenced but ultimately non-Marxist positions of thinkers such as Cole and Laski. These sections are often interesting and useful to historians of socialist ideas. The total effect, however, is kaleidoscopic and fragmentary. His interesting material is truncated when it escapes the limits of his main point. Major issues, such as explaining why Marxism failed to make significant inroads into the British labor movement, are touched on but not pulled together, and there is no real justification for his refusal to carry any doctrinal controversy past 1933.

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DAVID STEVENSON. *Alasdair MacColla and the Highland Problem in the Seventeenth Century*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1980. Pp. ix, 324. \$37.50.

In this attractive study, David Stevenson marks his first major venture into the Gaelic world of Ireland, the Western Isles, and the Highlands of Scotland with a valuable combination of clear analysis and

heroic narrative. The book begins with an excellent brief introduction to the Highlands and Western Isles in the early seventeenth century and ends with a more hurried overview of the "changing Highland problem in the later seventeenth century." In these topical chapters, Stevenson sympathetically charts the changes taking place in Gaelic society, instead of picturing it as essentially static and bound by ancient tradition, and expertly provides the context for his action-packed narrative.

The main subject, however, is Alasdair MacColla, a MacDonald warrior who attempted to revive the fortunes of his clan during the civil wars of the mid-seventeenth century by leading a band of fighting men from Ireland into the Highlands of Scotland. There they seized fortresses, linked up with the Earl of Montrose, harried the lands of the Campbells (the archenemies of the MacDonalds), and made up the experienced core of the royalist army in Scotland. The chapters on MacColla's deeds contain some stirring sections, not least because Stevenson, with the help of experts, draws upon translations of those dramatic Gaelic poems, stories, and accounts that relate these heroic exploits within a context shared by those who performed them. This results in a sympathetic but not sentimental portrayal of the mixed motivations and actions of MacColla and his warriors.

As in his previous books, Stevenson has carefully examined a wide range of sources and has used these to construct a convincing account that revises some of the work of earlier historians. For example, he takes apart S. R. Gardiner's account of the battle of Auldearn and puts together a version of that affair that far better fits the site and sources. He also demonstrates that MacColla played a much more significant, the Earl of Montrose a less important, role in the royalist victories in Scotland than previous historians have allowed. By showing concretely how MacColla and other Catholics felt threatened by the apocalyptic visions of English and Scottish Protestants, he contributes a major insight into the dialectic of that religious warfare that fueled the fears and emotions of many participants on all sides during the Great Rebellion. He argues that MacColla invented the tactic of the Highland charge and, in this and other ways, helped to revive the glory of Gaelic arms. A detailed examination of the impact upon the Highlands and Western Isles of the recruitment for and return from foreign service which was so widespread in early seventeenth-century Scotland, however, must precede a full assessment of MacColla's military contributions.

In short, then, Stevenson's book gives the reader a new perspective on the royalist victories and defeat in Scotland, a fresh look at the impact of another local community upon the events of the Great Rebellion, and some concrete illustrations of the com-

plex role of religion in that conflict. Both specialists and general readers should enjoy its judicious fusion of analytic and narrative history.

PAUL CHRISTIANSON
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KENNETH M. BOYD. *Scottish Church Attitudes to Sex, Marriage and the Family, 1850-1914*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1980. Pp. ix, 401. \$45.00.

Family history is booming, the subject of many volumes and the theme of meetings such as the 1980 Social History Society Conference in York, England. Not much of this modern work on the family has been marked by familiarity with the ecclesiastical world that so affected the culture of sex and family life in past time. It is a particular merit of Kenneth M. Boyd's book that he unites historical expertise with theological knowledge; he has given us a thorough and rewarding study of Presbyterian church attitudes, pronouncements, and discipline from the middle of the nineteenth century to the First World War. It is an important contribution both to our understanding of the Scottish Presbyterian past and to secular social history.

Although Boyd's principal research concerns the sixty years following 1850, he digs much deeper, beginning with sixteenth-century Presbyterianism, against which the disintegration of church discipline in moral (notably sexual) matters is measured and assessed. The changes in the centuries down to 1914 were indeed striking, brought on by a hugely increasing population, ecclesiastical divisions, and a marked softening of Calvinist rigidity in the eighteenth century. Yet amid the demographic explosion, rapid urbanization, and the disintegration of rural communities, the nineteenth-century churches (all of the principal Presbyterian denominations) appealed longingly to a mythical figure of rural virtue, a compound of pious peasant and noble savage, neatly dubbed by Boyd "sanctified MacRousseau" (p. 171). The trouble was that the species MacRousseau was rare to the point of extinction, a type of character remote from the real world of swinging "bothies" (rural doss-houses) and shockingly high illegitimacy that caused much anxiety to the Scottish middle classes.

The second half of Boyd's book consists of two parts, essentially separate studies that nevertheless fit well under the general title. The first (part 2 of the volume) is an interesting account of the Free Church and United Presbyterian parts in the campaign to repeal the Contagious Diseases Acts (a campaign in which the Church of Scotland, like the Church of England, played a more ambiguous

role). The third part of the book covers the roles of the Scottish churches in opposing the continual efforts from the mid-1840s until 1907 to legalize marriage with one's deceased wife's sister. Boyd's account of this extraordinarily persistent controversy reveals a remarkable Presbyterian firmness against liberalization (although there was softness among the United Presbyterians), a sharp contrast with non-Anglican Protestants in England.

BRIAN HEENEY
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EMMET LARKIN. *The Making of the Roman Catholic Church in Ireland, 1850-1860*. Chapel Hill: University of North Carolina Press. 1980. Pp. xxiv, 520. \$24.00.

The sectarian divisions that so bedevil Ireland did not appear overnight. Irish society traveled a long way between the battles of Clontarf and of the Bogside. One of the most important signposts along this very rough road was the convening of the synod of Thurles in 1850, at which the Roman Catholic hierarchy set the tone for ecclesiastical policy that prevailed, in spirit if not in all details, for at least the succeeding hundred years. This is the fourth book to appear in Emmet Larkin's fine series on the Roman Catholic Church in the nineteenth century, and it is much the most impressive he has thus far published.

Larkin's approach is that of an institutional biographer, telling with painstaking chronological detail the activities of the Catholic bishops. His mastery of primary materials is extraordinarily deep and his writing clear and compelling. Like most biographers, he is probably a bit too uncritical of his subject, but that is a minor flaw. His theme in this volume is that during the 1850s "as a body, the Irish bishops became whole, and the modern Irish Church was, in effect, made" (p. xxiv). This is a very generous way of describing a vicious power struggle within the Irish hierarchy. On the one side was the ultramontanist Archbishop Cullen, who faced, and gradually crushed, the erratic nationalist Archbishop MacHale. Cullen's victory was hard, cruel, and complete. To say that the Irish bishops were "made whole" (p. 482) by this process is tantamount to denominating Stalin's work in the USSR as healing and irenic.

The long intrachurch battle had its side effects, and one of these was that the church became committed to a rigid sectarianism in social matters, particularly in education. The Catholic bishops became wedded to a belief in segregated education (an anachronistic term, but appropriate). At the synod of Thurles the bishops condemned the new Queen's Colleges, which were nondenominational university colleges. The principle of mixing Catho-

lic and Protestant children in elementary and secondary schools was also condemned in the 1850s and 1860s, except in instances where the Catholic clergy had complete control of the schools in question.

Further, it was during this era of internecine strife that the Catholic Church in Ireland acquired its extreme fear of government of any sort. Cullen himself believed in a governmental "conspiracy to destroy the faith of Irish Catholics" (p. 101). This fundamental distrust of the state continues to this day, even in the overwhelmingly Catholic Republic of Ireland.

This book is fascinating reading, and if one may occasionally differ from Larkin in points of detail and interpretation, one is nevertheless grateful for his providing us with an insight into one of the most important periods in Irish religious history.

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PATRICIA JALLAND. *The Liberals and Ireland: The Ulster Question in British Politics to 1914*. New York: St. Martin's Press. 1980. Pp. 303. \$27.50.

In this study of the Irish Question and British politics in the period just before World War I, Patricia Jalland contends that the Liberal government's ineptitude was mainly to blame for the Home Rule crisis and that the Liberal party was seriously hurt as a result. Up to a point, her thesis is convincing. Jalland shows that the government was woefully unprepared to deal with Ulster's opposition to Home Rule in 1911-12. Thanks largely to the imperturbability of Prime Minister Asquith and the indecision of the Chief Secretary for Ireland, Augustine Birrell, the cabinet continued to ignore the Ulster problem until it had grown to dangerous dimensions, when compromise had become compelling but also much more difficult to achieve. The government's efforts to resolve the issue were clumsy and vacillating, and the United Kingdom was on the brink of civil war when the European conflict intervened, temporarily shelving the Irish Question. Jalland concedes that an early proposal to exclude Ulster from Home Rule might not have averted political deadlock, but she argues cogently that such a bold initiative would have weakened the Unionist opposition and helped the government retain control of the situation, whereas a policy of drift robbed the government of its options and reduced it to political bankruptcy.

The author is less persuasive in assessing the effect of the Irish fiasco on Liberal fortunes. The discontent produced by the Home Rule imbroglio seems much less important as a cause of the Liber-

als' decline than the impact of World War I. Nor is it clear that preoccupation with Ireland prevented the enactment of reforms that would have enhanced the party's electoral appeal. Even without Ireland, Liberal divisions over social questions and armaments might well have precluded further assaults on privilege in the years 1911-14.

Jalland's "politics from inside" approach has both strengths and weaknesses. Concentrating on Westminster, she provides a detailed analysis of high-level policy and maneuver on a vital issue, effectively utilizing the private papers of leading politicians and other primary sources. Her criticism of Liberal ministers, especially Asquith, is well-founded, although her emphasis tends to obscure the fact that the government's difficulties were partly due to the Conservative party's active encouragement of unconstitutional opposition to Home Rule. And, although her discussion of Ulster Unionism is adequate, she says little about the Irish nationalist movement, whose opposition to partition she almost certainly underestimates. Within the limits she has chosen, however, Jalland has written a penetrating and provocative study of British Liberalism and the Irish Question.

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JONATHAN DEWALD. *The Formation of a Provincial Nobility: The Magistrates of the Parlement of Rouen, 1499-1610*. Princeton: Princeton University Press. 1980. Pp. xv, 402. \$25.00.

The province of Normandy has attracted a number of noted French historians, perhaps because of its closeness to Paris. They have included Henri Pren-tout, Edmond Esmonin, Roland Mousnier, Paul Logié, Madeleine Foisil, and now Jonathan Dewald, who has written an excellent study of a sixteenth-century elite, the judges or robe nobles of the parlement of Rouen. Dewald has challenged the traditional view, held by Mousnier among others, that the military or sword nobility and the magisterial or robe nobility were antagonistic, conflicting elites in the sixteenth century. Dewald believes that their similarities were greater than their differences and that the two became a reasonably cohesive, landed elite beneath the great old families of the feudal nobility. Robe and sword in sixteenth-century Normandy had similar family connections, economic resources, and ways of estate management (p. 309).

In a well-researched and clearly written book, Dewald argues that the numbers of robe nobles increased during the sixteenth century, while the geographical range of their recruitment diminished. Most came from within the province and from the

minor sword nobility, the country gentry, and officials. This pattern of recruitment was responsible for one-half of the *parlementaires* being nobles of long standing by the end of the century (pp. 109–10). Robe nobles also became wealthier during the century: their fortunes increased tenfold, and they were ranked as poorer members of the high sword nobility by the end of the century (pp. 158–59). Land was the most important part of their fortunes, as it was for the sword, and they managed it in much the same way (pp. 219–20). Their economic behavior was calculated upon material interests, not upon a capitalistic approach to profit (p. 311). Their family fortunes fluctuated in bursts, up and down, during the century rather than showing a steady rise, and they increasingly went into debt. Thus, robe nobles could not afford to dower their daughters adequately as the century progressed and tended to marry them into the lesser gentry of the countryside (pp. 271–72). Economic factors became more important in marriages, and a large dowry, or the patronage of a great noble achieved through marriage, could send a family's fortunes skyward (pp. 275, 285).

Having studied the Aix *parlementaires* of the early seventeenth century, I tend to agree with Dewald's basic conclusion that robe and sword were closer and more cohesive than we have realized. With all of its merits, however, Dewald's book has several problems. He recognizes that differences existed between robe and sword, but he attributes conflicts, which he admits were sometimes intense and violent, to differences between individuals and professional groups, not between hereditary social orders (pp. 107–08, 111, 309). Although admitting that conflicts existed, Dewald does not devote a separate section to analyzing or discussing these. He summarily dismisses them as a professional, attitudinal split not indicative of deeper, more fundamental differences, and he offers a mass of quantitative economic and social evidence on similarities. I am inclined to agree with him, but I must know more about these differences, as contemporaries perceived them, before I can. Much of the evidence for robe-sword hostility comes from literary sources—there are some good examples at Aix in the 1649 *mazarinades*—and Dewald has eschewed these in favor of hard, quantitative evidence (pp. 3, 8). In so doing, he has weakened his argument by omission. Social hostilities have a long life as attitudes after the conditions that created them have disappeared, and this makes them no less real.

The other problem I have, with Dewald's argument is his failure to examine in depth patron-client relationships between the robe and the great sword nobility of the province. He alludes to these ties in several places in the book, for instance, in connection with recruitment (pp. 86–94), landowning

(pp. 178–79), and marriage (pp. 257, 285–90), but nowhere does he make them a separate topic for discussion. He announces early in the book that "what was critical to most robe careers at Rouen was not loyalty to the crown but patronage from members of the great aristocracy" (p. 86). If this type of patronage was critical, it deserves more space than Dewald has given it. Its nature and details should not be taken for granted. My own studies at Aix in a later period make me wonder exactly how extensive and important were patron-client ties between the robe and the provincial feudal nobility. These criticisms, however, should not detract from the real achievements of a fine book. Dewald has done an impressive amount of research in notarial records on the incomes, landowning, estate management, and marriages of the Rouen *parlementaires* in a neglected period, and he has created a vivid picture of the social and economic aspects of their daily lives, which I found a pleasure to read. The book adds greatly to our knowledge of the robe nobility in the sixteenth century and should become standard reading on the period.

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ROBERT LOUIS STEIN, *The French Slave Trade in the Eighteenth Century: An Old Regime Business*. Madison: University of Wisconsin Press. 1979. Pp. xvii, 250. \$20.00.

The last decade has seen emerge a number of works suggesting that French business practices prior to the Revolution reflected more the archaic structures forged in the Renaissance than those familiar to students of nineteenth-century capitalism. Not least among these is the influential *French Finances, 1770–1795: From Business to Bureaucracy* (1970) by John Bosher, under whose guidance Robert Louis Stein wrote his Ph.D. dissertation. Bosher and others argue that business in the eighteenth century was characterized by the persistence of family concerns and short-lived associations, by the absence of modern financial structures and accounting techniques, and by a conservative outlook and close links with the absolutist state. Precisely because the slave trade has been seen by some (such as Eric Williams) as a significant contributing factor to the emergence of the modern industrial economy, Stein's study of this important sector of the fast-growing French maritime economy proposes to buttress what may be labeled the Bosher thesis.

Not surprisingly, since Nantes dominated the French slave trade in the eighteenth century, much of the author's documentation comes from this port, but it is supplemented by wide-ranging readings on

other ports and some research in their archives. As such, *The French Slave Trade* must be welcomed as the most up-to-date overview of the topic. In a very brief form, and despite some errors and a number of debatable assertions, it will serve as a convenient guide to the more specialized studies found scattered in a plethora of local monographs and articles. On the whole, however, Stein does not replace these studies, which on particular points are much to be preferred. The statistical tables found at the end of the volume will be quickly superseded as a result of the publication, begun in 1978, of Jean Mettas's detailed list of slaving expeditions. Similarly, the specialized literature on the subject is still to be preferred to Stein's treatment of the mortality rate among slaves shipped across the Atlantic and its decline over the century. More crucially, since the matter bears directly on the central thesis of the work, the masterly analyses of the profitability of the trade and the structure of slaving associations found in Jean Meyer, *L'Armement nantais dans la deuxième moitié du XVIII^e siècle* (1969), are not faithfully reflected in the present study.

The author comes to grips with his thesis in the last seventy-odd pages of the work (part 3, "The Traders and their Business"). The brevity of this section makes one regret the space devoted to background in the first two parts, which perhaps reflect too much the origins of the study as a dissertation. Despite much useful information and some newly discovered details, the material found in part 3 does not adequately support the author's conclusions concerning the slave traders' "attachment to the old forms of partnership" and their remaining "a true part of the old regime . . . in the tradition of medieval merchants more than of modern industrial capitalists" (pp. 201-02). One may regret as well the lack of concrete evidence about the investment networks that are purported to have existed between port *armateurs*, Parisian financiers, and foreign investors. The point is important, as it serves to buttress the "old regime" characterization of the trade. It is also where Meyer's study left off, and where, consequently, one would expect a newer work to make an important contribution.

At times, the evidence presented even militates against the thesis. In an important section, Stein discusses the many slaving merchants who achieved *nobiliar* status and concludes that their social elevation had little effect. They continued in the trade, "remained merchants at heart and maintained a system of values far removed from the aristocratic one" (p. 191). In this respect at least, the men whom Stein studies might be defined as more akin to their English counterparts than to their similarly placed compatriots.

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J.-D. DE LA ROCHEFOUCAULD *et al.* *Le duc de La Rochefoucauld-Liancourt, 1747-1827: De Louis XV à Charles X, un grand seigneur patriote et le mouvement populaire*. Paris: Librairie Académique Perrin. 1980. Pp. 457.

Army officer, courtier, enlightened *seigneur*, philanthropist, legislator, gentleman farmer, author, and exile: François Alexandre Frédéric duc de La Rochefoucauld-Liancourt (1747-1827) was all of these and more. Scion of an old noble family, he became Grand Master of the Wardrobe in 1768 and began a military career. But he preferred to devote himself to his estates and agricultural experiments. He sought to remedy widespread rural poverty by establishing cottage industry, later expanded to factories, to provide employment and by opening a school of arts and crafts that evolved into a national institution.

In the Estates General he supported the monarchy but advocated conciliation and reforms such as equal taxation. In 1792 he assumed command at Rouen. When his plan to rescue the king foundered he fled to England, where many émigrés regarded him as a traitor for his moderation. He then traveled widely in the United States. After 18 brumaire La Rochefoucauld reappeared in France, recovered some of his lands, and expanded his industries, earning appointment to the council on manufacturing. He also promoted vaccination, winning government support and publicly inoculating the king of Rome.

In the Restoration's Chamber of Peers he generally supported moderate and liberal positions. He was inspector-general of the conservatory of arts and crafts and served on councils on agriculture, asylums, manufacturing, prisons, and vaccination. He continued his private humanitarian efforts, establishing mutual aid societies in his own factories, creating a savings bank to aid workers, and supporting the mutual education movement. But in 1823 his opposition to the cabinet cost him all posts except his peerage.

The present biography is highly complimentary to La Rochefoucauld, stressing the philanthropic, social, and other accomplishments that assure his place in history. But he cannot be considered a man of great political importance. The merit of this book, which is based largely on La Rochefoucauld's own papers, is its valuable study of the man and his times, especially its insights into such things as his operation of a noble estate in the Old Regime and his support of popular reform. Unfortunately there are occasional weaknesses of scholarship. Secondary works are used with annoying frequency as sources for documents and even for printed materials, and no sources at all are given for some lengthy quotations. The strength of the work, its basic interpretations, and its most important contributions are

found in those chapters covering 1747 to 1792. It is well worth reading.

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TERRY SHINN. *Savoir scientifique et pouvoir social: L'école polytechnique, 1794-1914*. Preface by FRANÇOIS FURET. Paris: Presses de la Fondation Nationale des Sciences Politiques. 1980. Pp. 261. 78 fr.

This book is divided into two parts. The first and principal section is a history of the *École Polytechnique* (EP) from 1794 to 1914. It deals with the foundation and evolution, to 1830, of this famous school; the selection, discipline, and training of the students; their social background and postgraduate careers during the school's apogee, 1830-80; and finally with the same topics during three decades of transition before the First World War. Despite its resistance to change, the EP is shown to have become somewhat less of a bastion of the *haute bourgeoisie* by 1914 than it had been in 1880. The second section of the book, an *annexe*, consists of a thirty-page essay tracing the evolution of the French engineering profession from one concerned almost exclusively with military and state constructions (roads, bridges, mines) to one engaged in the multiple tasks (electricals, chemicals, mechanicals) demanded by the dynamic capitalistic economy of the late nineteenth century. The book's two sections have a common theme because the EP, rooted in the state service tradition, opposed the transformation of engineering into a modern profession. Its obstructionism failed, however, because the university faculties of science successfully educated the needed engineers.

The book is well researched and well written. Terry Shinn's statistical work on social background and postgraduate careers is especially well done; his essay on French engineers presents a complex topic lucidly. At the risk of disparaging a good book, however, something should be said about its weaknesses. First, Shinn notes that the EP made every effort to turn its students into a self-conscious elite, into *Polytechniciens*. Much of the book, however, is about the students' social origins. Did the conditioning in school produce the same *Polytechnicien* regardless of social origins? Shinn suggests as much. If so, why document social origins so thoroughly? It cannot be that the EP inculcated elitist attitudes directly into French society. The school only took 200 students a year. Second, Shinn states that most *Polytechniciens* held military and state bureaucratic positions. Elsewhere he chastises them for having made few major contributions to mathematics and science. But he does not evaluate them as administrators. Did they make any contributions to the science

of state or business administration, or were they chiefly responsible for the shortcomings in French military and state management in 1914? Third, Shinn's evaluation of the EP is not as original as he thinks. He has, in fact, had the misfortune of encountering in this reviewer one who made many of the same points about the social and educational deficiencies of the EP in an article, which is not cited, published three years ago.

Still, nobody has investigated the *Polytechniciens* as thoroughly as Shinn. He has made a solid contribution to our knowledge of an important subject.

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GUY THUILLIER. *Bureaucratie et Bureaucrates en France au XIX^e siècle*. Preface by JEAN TULARD. (Hautes Études Médiévales et Modernes, number 38.) Geneva: Librairie Droz. 1980. Pp. xix, 670.

The team of Jean Tulard and Guy Thuillier is at work again. In the preface to this volume, which is not really on the nineteenth century since it includes nothing on the Napoleonic bureaucracy, Tulard outlines the value, needs, and methods of the history of government administration. Thuillier follows the same approach in the text for the period 1840-1940, emphasizing types of sources and presenting only preliminary analysis rather than finished historical writing. Beyond Tulard's preface no attempt is made to provide a unifying focus for the six parts of this lengthy volume.

Part 1 of the book includes nine essays dealing with individual bureaucrats' recollections or criticisms, ranging from Guy de Maupassant's difficulties with bureaucratic routine to Eugene Aimé's frustrations as a conscientious, thoughtful, yet powerless underling. In part 2 Thuillier provides a brief history of the administrative press from 1840 to today, although it is more a discussion of potential sources than an actual history. Promotion, notation, and discipline are covered in seven essays in part 3, the best section. Topics of enduring interest are discussed here: promotion by merit or seniority, the communication of personnel dossiers to the employee, and the use of competitive examinations for recruitment and advancement. Part 4 has three short essays on early proposals for a national school of administration, while part 5 is an essay on the changes in daily behavior at the office. The last 100 pages are actual documents relating to the people and issues discussed in the text.

Although one's interest in administrative history is aroused by this book, I wonder how many people will benefit from it. For the specialist there are documents and a guide to sources; for the occa-

sional reading of a student there are individual essays of interest; for the general historian, however, I find neither an overall picture of the subject nor specific insights on general topics. For example, Aimés served in the Ministry of War from 1840 to 1885, but Thuillier does not discuss the survival of bureaucrats across regimes.

This book brings home how little we know about bureaucracy. While Thuillier's effort enhances our knowledge, generally I feel his energy would have been better spent in writing an actual history of administration or in writing complete essays rather than vignettes on specific aspects of administration. How I yearned to read a full account of the practices and controversy surrounding promotion. After all, the problems encountered by bureaucrats in the nineteenth century are still with us today.

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MICHAEL STEPHEN SMITH. *Tariff Reform in France, 1860–1900: The Politics of Economic Interest*. Ithaca: Cornell University Press. 1980. Pp. 272. \$18.50.

This work fills the gap between Arthur L. Dunham's *The Anglo-French Treaty of Commerce of 1860* (1930) and Eugene O. Golob's *The Méline Tariff* (1944), as it focuses upon the period 1878–82 and deals with the elaboration of the Tirard tariff of 1881 and the cluster of trade treaties that it spawned. It is also a revisionist work aimed at establishing, *contra* Dunham, that the interests favoring freer trade were stronger than generally conceded and that they managed to preserve the 1860 system until the 1890s. In addition, Michael Stephen Smith argues, *contra* Golob, that the Méline tariff was a moderate compromise between liberals and protectionists, an abandonment of bitter struggle in favor of "interest accommodation." In this rendering Jules Méline is transformed from a hellbent protectionist into a judicious statesman. Consensus having replaced conflict, the commercial and industrial bourgeoisie of the Third Republic finally was united, the tariff settlement helping to achieve what Smith calls the "organization of capitalism." "Thus, thanks in large measure to the tariff reform of the 1890s and the accommodation of economic interests, the capitalist elite was able to preserve its political, economic, and social supremacy in France despite the discrediting of specific bourgeois politicians in the scandals of the *fin de siècle* and despite the growing alienation of the working class and the resultant growth of the socialist and syndicalist movements" (p. 241).

Smith presents an extensive and perceptive analysis of the relative strength of both the liberal and

protectionist camps in the 1870s, an analysis that convincingly supports his contention concerning the continued dominance of the freer trade interests into the 1870s (as against the view that the French hastened to return to high protection as soon as possible after the collapse of the Second Empire). Smith expertly leads the reader through the long debate preceding the passage of the tariff of 1881, including the formation of rival protectionist and liberal associations to defend those interests. This is the heart of Smith's book, and it constitutes an important and original contribution.

The concluding chapter covers the reversal of French tariff policy, which was prepared in the 1880s and triumphed with the passing of the Méline tariff of 1892. This was possible because most agricultural interests, still a potent force, shifted in the 1880s from a liberal stance to protectionism. It is difficult to credit Smith's thesis that the Méline tariff represents a compromise, an "accommodation of interests" of different factions of the bourgeoisie, achieving the "organization of capitalism" in France, and establishing patterns that persisted through the remainder of the Third Republic. These labels conceal too much, and to support them Smith regards as "moderate" tariff rates that have been regarded generally as "high." If the losers in the tariff reversal "accommodated," what other choice did they have? Some of them "accommodated" by going out of business. And it may be doubted that the French bourgeoisie was any more united or free from intraclass divisions and conflicts after the passing of the tariff than before. Old issues may have waned, but new ones appeared to take their place.

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RAINER HUDEMANN. *Faktionsbildung im französischen Parlament: Zur Entwicklung des Parteiensystems in der frühen Dritten Republik (1871–1875)*. (Beihefte der Francia, number 8.) Munich: Artemis Verlag. 1979. Pp. 477. DM 128.

Rainer Hudemann demonstrates a high order of contemporary analysis in his study of the way what amounted to a party system developed in France in the days of the National Assembly of 1871–1875, especially after MacMahon replaced Thiers as president. This Trier dissertation is among the special publications of the journal *Francia*, and both reflect favorably on recent German scholarship on France.

Hudemann analyzes with care the major and minor groups as they took shape in the National Assembly. His table of the formation, duration, and relationships of these groupings is remarkably lucid

(p. 31). In drawing clear lines, however, he does not fail to introduce the complications of economic interest that caused overlapping of purposes and double memberships. Indeed, his analysis and chart of these double memberships are among the best parts of his study (p. 134).

Hudemann tabulates the behavior of these groupings in both the great constitutional questions and in practical matters of the day, such as military, tax, and church and state matters. He shows the increasing stability of the groupings and something at times approaching bloc voting of most of them. He interestingly demonstrates, however, that the Bonapartists and the Center Left were less reliable on the whole than the others. In his conclusions on these things Hudemann is convincing without being insistent. He treats the National Assembly with reference to modern parliaments in other countries as well as France.

Hudemann's appendixes are particularly informative. His membership lists for the different groupings show overlaps and a variety of facts about the parliamentarians. His analyses (pp. 275-90) of a wide variety of votes clearly buttress his thesis. His unannotated bibliography is overwhelming.

Among the appendixes are statements of general political positions represented by the groupings. These statements seem to this reviewer to breathe the spirit of the *reunions* of that day better than all the statistics a scientific researcher can assemble and correlate. This period was exactly the time when Manet showed the merit of impressionism in painting. The words of Thiers, Chambord, and Gambetta tell us more about political life of their day than do the cold facts we have here. The refusal of Athanase de Charette (who is not mentioned) to accept election is perhaps more significant than the percentage of *chevau-léger* votes on a given issue.

Hudemann did not set out to recreate in a humanistic way an atmosphere already amply studied elsewhere. What he has done is give an analytical account of nascent party growth in France that offers a supplement in numerical precision to knowledge and understanding of the period.

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JACK D. ELLIS. *The Early Life of Georges Clemenceau, 1841-1893*. Lawrence: Regents Press of Kansas. 1980. Pp. xix, 272. \$20.00.

Georges Clemenceau is one of the most difficult subjects in modern French history that a biographer can tackle, and the difficulty is particularly great for his early life. Clemenceau was a man of violent extremes, and his biographer has to account for the differences between the defender of amnesty for the

Communards and the strike-breaking prime minister, between the defender of Dreyfus and Père-la-Victoire. Jack D. Ellis's decision to stop at the nadir of Clemenceau's career is not an effort to evade this difficulty but rather to concentrate on that aspect of Clemenceau's behavior that he finds most revealing: the apparent contradiction between the Tiger's espousal of radical causes and the counterproductive manner in which he chose to promote them.

That contradiction can be understood, according to Ellis, through the developmental psychology of Erik Erikson. Clemenceau's whole career is thus explained in terms of his inability to make a satisfactory transition from the phase of identification with his father to the phase of asserting his own ego identity. Clemenceau's radical politics is presented as an attempt to secure his father's approval by the advocacy of the latter's ideas and even by the replication of the suffering for those ideas that his father had experienced. Every failure to live up to this standard generated a feeling of guilt in Clemenceau, but the very effort to succeed was felt as an alienation of self, a threat to the development of his own ego. This counterimpulse accounts for Clemenceau's choice of methods that were almost certain to fail: his inability to compromise, his disdain for organization, and his penchant for alienating potential supporters. Both forces were so strong that as long as his father lived neither could triumph.

Ellis produces considerable evidence to support this interpretation of Clemenceau's behavior, but, inevitably, he occasionally falls back on direct references to psychological theory to fill the gaps in his information. Even when the resulting interpretation seems plausible one wishes for more concrete evidence—evidence we are not likely ever to have—that Clemenceau felt in fact what the theory requires. We should certainly not forget that more traditional approaches to biography are subject to the same failings and that unconscious psychologizing is not more dependable than the conscious psychologizing of which this work is a fine example. The fundamental question for the historian, however, remains unsolved: how legitimate is the leap from a theory that is meant to explain human behavior in general to the analysis of an individual whose conformity to the model is problematic?

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ISABEL BOUSSARD. *Vichy et la Corporation paysanne*. Foreword by RENÉ RÉMOND. Paris: Presses de la Fondation Nationale des Sciences Politiques. 1980. Pp. 414.

The Corporation Paysanne, established in 1940 by Vichy France, embodied two of the principles most

cherished by the new regime: corporatism and agrarianism. The Corporation Paysanne symbolized both Vichy's return to the soil and its hostility to the parliamentary politics of the Third Republic. The new corporation was to be the self-governing professional body of the peasantry—independent of the state and free from the manipulation of the politicians. As Isabel Boussard's detailed study of the Corporation Paysanne clearly shows, these expectations were rarely realized. For all its lip service to corporatism, the Vichy government seldom granted the corporation the autonomy it desired. The functionaries of the Ministry of Agriculture tended to treat the corporation as a group of meddling amateurs whose pretensions far exceeded their limited competence. It required over two years to organize the corporation and even then it remained financially dependent upon the state, owing largely to its inability to collect membership dues. Both the Resistance and the fascist right contemptuously dismissed the corporation as a band of "hobereaux." In the end, the principal task of the corporation came to be the extremely unpopular one of apportioning the obligatory contributions of the rural producers to feeding the cities and the Germans—an ironic fate for a theoretically independent agrarian organization. The corporation ultimately achieved little under Vichy, and its greatest accomplishment, as the author carefully demonstrates, lay in developing the cadres who would staff the agrarian organizations of postwar France.

Boussard's book is based on the recently accessible archives of the corporation as well as those of the relevant ministries and is carefully researched and clearly written. It is, nonetheless, narrative institutional history written for the specialist. Little background information is provided, and Boussard takes for granted that a reader will readily recognize the RNP as Marcel Déat's *Rassemblement national populaire* and be alert to its significance. Her habit of referring to organizations by their street addresses will irritate even specialists who are forced to leaf back and forth trying to establish which of several prewar agrarian organizations the "rue des Pyramides" refers to. Documents are quoted at great length throughout the book, making at times for heavy going. Periodically these extensive citations give an intriguing glimpse into the inner workings of the corporation, but too often they are simply formal policy statements or banquet addresses that could be (and, in fairness, usually are) summarized in a sentence or two. Finally, this is a curiously apolitical work. Although she occasionally quotes contemporaries to the effect that the corporation represented primarily the interests of "les grands seigneurs," the implications of such suggestions are never explored. Similarly, in a book of this length it

is a little surprising not to find a more systematic treatment of Henri Dorgères, leader of the prewar protofascist Green Shirts and active in the corporation. The book has all the advantages and drawbacks of history written from the inside. Boussard has given us by far the most thorough account of the workings of the Corporation Paysanne, but one cannot help wishing that her perspective had been a more critical one.

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MARY ELIZABETH PERRY. *Crime and Society in Early Modern Seville*. Hanover, N.H.: University Press of New England. 1980. Pp. viii, 298. \$14.00.

Research on crime in early modern Spain remains an almost unexplored field of investigation due principally to a paucity of existing records. Although the available material, fragmentary and scattered, does not lend itself easily to statistical analysis, it is still essential to any serious study of crime in this period. Mary Elizabeth Perry purports to investigate crime in early modern Seville (sixteenth and seventeenth centuries) without including this extant data. Instead she bases her book on administrative reports, chronicles, literary sources, and contemporary writings. Although this material is intrinsically interesting and valuable, it is by its nature anecdotal; hence, the book becomes a collection of anecdotes piled upon anecdotes. More damaging is that this book abounds in errors of all kinds, including faulty translations of sources, misinterpretations, and oversimplifications. It also lacks a real understanding of the early modern period and suffers from present-mindedness. For want of space in a short review I shall present only a selection of some of these errors. Others may be found on almost every page of the book.

The author mistranslates and misinterprets several selections from one of her principal sources, Cristóbal de Chaves's *Relación de la cárcel de Sevilla*. On page 85 she presents what she claims are the rites followed by women in the jail of Seville when a prostitute is condemned to death. In reality, this excerpt describes the death of a male prisoner who is attended in his last moments by his prostitute girlfriend and other women from the brothels. The Spanish text reads: "Quando se sabe en la mancebía ó en la casa de la muger que tiene por amiga el que ha de morir, viene acompañada de otras semejantes á la prisión" (Chaves, part 2). On page 218 the author discusses the vulnerability of female servants to their masters. She uses another selection from Chaves in which she says that a young woman ser-

vant is seduced by the son of her master. The Spanish texts read: "un Fulano de Molina . . . se le averiguó haber sacado de la casa de su padre una doncella . . . [y] la engañó" (Chaves, part 2). This episode clearly does not bear any relationship to servants and masters.

In sixteenth- and seventeenth-century Spain clergymen and others ministering to the spiritual and material needs of prisoners can hardly be faulted for "not promising them a better world or a more just social order" (p. 82). Perry states that the records of the charitable dowries granted by Juan de Mañara from 1660–70 are particularly valuable for a study of the underworld, but she does not submit them to a systematic analysis.

The author has a simplistic concept of the underworld. Although it is true that in Seville as in other early modern cities there existed a group of professional criminals, extant judicial records show that most criminals were not professionals or marginal people. Perry tries to establish a significant role for the underworld in the revolt of 1521 and 1652, but her sources do not support this. Finally, all of this is written in rather awkward prose with an abundance of slang expressions in addition to errors in the spelling of Spanish names and carelessness in accentuation.

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JOSÉ ANTONIO MARAVALL. *Poder, honor y élites en el siglo XVII*. (Historia.) Madrid: Siglo Veintiuno Editores. 1979. Pp. 310.

With his usual erudition and grace, José Antonio Maravall discusses the complex interrelationship of political power, honor, and elites in seventeenth-century Spain in this collection of two distinct sets of essays. The theme of honor, discussed in part 1, has always exercised an extraordinary fascination for students of Spanish society in every period. But in the end the concept of honor itself and the social reality behind it have remained elusive for those interested in establishing historical certainties about Spain's old regime society. Statute books, plays, works of art, and government records testify to the reality well enough, but they do not explain it.

Maravall offers here a modern interpretation that draws upon modern sociological theory as well as upon the author's deep knowledge of seventeenth-century Spanish history and literature. Some of his conclusions will not surprise, such as the role of honor as an exclusionary principle in a society increasingly dominated by the nobility. But there are splendid insights as well. His discussion of the relationship of the concept of honor identified with the

nobility to that of purity of blood with its more popular roots admirably explains the enormous differences between these two important social laws governing early modern Spanish society. And his treatment of the conflict between Christian values and the demands of a social code based on honor (you should be "menos cristiano pero más honrado," said an aggrieved father to a timid son in an affair that brought dishonor to the family) provides a new perspective on the complete triumph of noble standards of honor by the seventeenth century. Part 2 covers more conventional ground, focusing on how a noble society justified its privileged situation in both state and society long after it had lost the warrior function that had once justified its existence.

The analysis is lucid and intelligent, although the general conclusions are familiar enough to students of early modern European society. Both sets of essays reflect the mature reflections of a lifetime of scholarship; they should evoke intelligent reflection in those fortunate enough to read them.

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MYRON P. GUTMANN. *War and Rural Life in the Early Modern Low Countries*. Princeton: Princeton University Press. 1980. Pp. xvi, 311. \$22.50.

Writing since the end of the Second World War, the French historians associated with the *Annales* have thoroughly renewed our knowledge of European society in the early modern period. Among their many accomplishments, surely not the least is the finding of a significant causal relationship between subsistence crises, epidemics, and demographic change. The *Annales* historians have slighted war, however, as a factor of population, partly owing to their overriding concern with fundamental structures, but also because war's incidence was too fortuitous, and its impact too problematical, to fit readily into the schema of the *longue durée*. Nor is there much sustained analysis elsewhere of the effects of military action on noncombatants, except in studies of Germany during the Thirty Years' War, the consequences of which were drastic, lasting, and sometimes irreversible.

In the book under review, Myron P. Gutmann examines the impact of warfare between 1620 and 1750 on the residents of the Basse-Meuse, a grain-producing and dairy-farming region straddling the Meuse between Liège and Maastricht in the eastern Netherlands. Through the extensive use of quantified data extracted from parish registers, tithe records, rosters of admission to bourgeois status, commodity price lists, and the like the author is able to correlate such factors as the volume of agricultural

production, the fluctuations of the grain price, and vital statistics with the occurrence of epidemics and the vicissitudes of war. Although the Basse-Meuse was afflicted as grievously by military action during the last three decades of the seventeenth century, according to Gutmann, as was Germany between 1618 and 1648, its inhabitants were better prepared to endure the sufferings of war. The rural people of the Basse-Meuse were legally free, unlike the German peasants, who often lived in bondage; many of them owned their own land and their farms. The soundness of the region's economy, and the people's stake in it, induced them to bear the ravages of war in the expectation of an improvement in their situation. The industries of Liège, moreover, provided alternate forms of employment for rural folk whenever a crisis gripped the countryside. The wealth of that city's clergy and bourgeoisie furnished the credit that was used to defray the exorbitant cost of forced levies during military occupations. It is the presence of these factors that explains the resilience of the Basse-Mosan population in the face of adversity, the author avers, and that enabled their economy to resume its advance in the early 1700s. The almost unbroken peace that the Basse-Meuse enjoyed during the first half of the eighteenth century explains why that region weathered the crisis of the War of the Austrian Succession in 1746–48 with only a temporary downturn in its population and productivity.

Gutmann has given us here a paradigm of social survival in the preindustrial age. The author's construct is not, however, free from defects, for all the rigor and acuteness of his argument. His parallel between the Basse-Meuse and Germany rests largely on inference, as does his linking of the spread of disease with the presence of foreign troops. Nevertheless, his model should now be tested in other areas that similarly experienced firsthand the depredations of war.

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EINAR MOLLAND. *Norges kirkehistorie i det 19. århundre* [Norway's Church History in the Nineteenth Century]. In two volumes. Oslo: Gyldendal Norsk Forlag. 1979. Pp. 408; 354.

The posthumous publication of these two volumes is unquestionably one of the most significant recent contributions to the historiography of Norwegian Christianity. Incorporating a wealth of material, which Einar Molland covered in university lectures spanning eleven semesters, they will tell most readers as much if not more than they want to know about Lutheranism in Norway during the water-

shed era they encompass. Molland's work supplements the pertinent sections of the standard *Norsk kirkehistorie* by Andreas Aarflot and Carl Fredrik Wisløff and supersedes his own sketchy *Fra Hans Nielsen Hauge til Eivind Berggrav*, rendered into English as *Church Life in Norway*. Richly documented and written in a lively—in places almost anecdotal—style, these final volumes of his extensive corpus promise to remain an important secondary work for many years. Their value is enhanced by the fact that Molland, unlike some of his Norwegian colleagues who have succumbed to the temptation to exploit ecclesiastical historiography by taking up the cudgel against their theological opponents and thereby burdened readers with unabashedly tendentious tomes, insisted on treating divergent parties sympathetically, at least those within the Lutheran fold.

The present study begins in 1814, a date appropriate for at least three reasons. First, it marked the end of official Danish hegemony over Norway; for the first time Norwegian Lutheranism was administered by indigenous churchmen and secular bureaucrats. Second, Hans Nielsen Hauge, the revivalist who inspired the lay movement so influential in Norway, was then near the apex of his career. Third, the institution later known as the University of Oslo had just begun to educate pastors for the state church. It became the arena for the fateful theological bouts that Molland recounted in considerable detail. The protracted strife yielded a *de facto* schism within the state church and led to the founding of the conservative Independent Faculty of Theology in 1908, the terminal date of the second volume.

Molland adequately describes the forces that transformed Norway from an utterly rural land with fewer than a million inhabitants at the beginning of the nineteenth century to a rapidly modernizing democracy by its close. Proceeding from this background, he analyzes the course of Norwegian Christianity in terms of change reflecting political and intellectual currents, especially the lay challenge to clerical prerogative within the church and the proliferation of theological liberalism after the so-called "breakthrough of modernity" in the 1880s. Ramifications of these two central themes fill several hundred pages. Other topics, most notably liturgical reform, the gradual unfolding of religious freedom, and the impact of Danish Grundtvigian theology, receive secondary attention.

Molland casts a wide but not comprehensive net. If, as he emphasizes, his study of nineteenth-century Norwegian Christianity is intended to illuminate factors which shaped that of the twentieth, one might legitimately ask why the antecedents of so many aspects evident today, such as Christian social work, millenarianism, and the Sunday school move-

ment, are ignored or given short shrift. These and other developments, often related to Anglo-American influences on Scandinavian church life, all have deep roots in the nineteenth century. Regrettably, Norwegian church historians have not yet given these phenomena their due.

Another caveat must be mentioned. This work would be more aptly titled "The History of the Church of Norway in the Nineteenth Century," for Molland focuses almost exclusively on what the Norwegian constitution defines as "the official religion of the state." Molland was a loyal son of the Lutheran state church and strove to preserve its official status. Indeed, in one of his final public acts, he helped to draft a dissenting opinion for an official commission that had voted to advise the parliament to consider a separation of church and state. Molland's attachment to the official national faith of which he was a pastor explains but hardly excuses his merely skeletal treatment of the denominational mosaic that became quite complex before 1900 and is presently a major force in Norwegian Christianity.

Despite these shortcomings, Molland's posthumous work is a sorely needed addition to the historiography of Norwegian Christianity. Although its cumbersome chapters and lack of a subject index militate against its use as a reference work, it should long remain a valuable tool for specialists and other dedicated readers.

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SEPPO HENTILÄ. *Den svenska arbetarklassen och reformismens genombrott inom SAP ööre 1914: Arbetarklassens ställning, strategi och ideologi* [The Swedish Working Class and the Triumph of Reformism in the S.A.P. before 1914: The Status, Strategy, and Ideology of the Working Class]. (Historiallisia Tutkimuksia, number 111.) Forssa: Forssan Kirjapaino Oy. 1979. Pp. 330.

The success of the Swedish Social Democratic party has long made it an object of scholarly study. The party dominated Swedish politics in the post-Depression era, serving in the government continuously from 1932 to 1976 with the exception of a single three-month absence. As early as 1917, the party joined a "left-coalition" government that completed the transformation of Sweden into a fully democratic, parliamentary state. The best-known study of the Social Democrats is Herbert Tingsten's *Den svenska Socialdemokratins Ideutveckling* (1941). Tingsten's work has set the tone for much that has followed it. Tingsten himself was a major intellectual and political figure of the years 1940-60 in Sweden. He began his academic career as a So-

cial Democrat but slowly moved into the radical-liberal camp. His work was as much an intellectual memoir of his own rite of passage as a careful study of the ideological development of the party. Yet the study is so rich that no scholar can ignore it. Tingsten argues that the Social Democrats began to alter their revolutionary ideology around the turn of the century and finally gave up all pretense of ideological consistency in the thirties. Tingsten was a major contributor to the "end of ideology" school in the fifties.

Seppo Hentilä's study complements Tingsten's intellectual history by seeking the social, economic, and political bases for the shifting ideological attitudes of the party in particular and the labor movement in general. It is an ambitious project. Whereas Tingsten suggested that the primary factor contributing to the transformation of the Social Democrats was the progressive democratization of Sweden after 1905, Hentilä believes that the change reflected an improvement in the social position of the working class or, in reality, an improvement in the position of the "labor aristocracy" that dominated leadership positions in the labor movement. The two views are not antithetical.

Hentilä's study provides a solid overview of the labor movement in 1900-14. Numerous tables offer helpful information on wages, social conditions, occupational distributions, and so forth. He convincingly illustrates the dominance of the best-situated labor groups, that is, "the labor aristocracy," in the labor movement and chronicles the gradual shift toward reformism between 1902 and 1914. Additionally, his analysis of the influence of Karl Kautsky is very stimulating. Hentilä sees the seeds of reformism firmly implanted in Kautsky's version of Marxist orthodoxy. Swedish Social Democrats maintained both a commitment to working within the established system as well as the belief that revolutionary action would eventually be necessary. Hentilä posits that the party dropped this latter perspective by 1914, hence entering full-fledged the traditions of reformism without being fully aware of the implications of its behavior.

The book is not smoothly written. Organization is awkward. The author's own Marxism leads to the use of some "key words" that do not aid in clarification of his themes. His macroconceptualization forces him to avoid analysis of the specific interplay between the labor elite despite his own recognition that decision-making authority became highly centralized within the labor movement. The importance of the collapse of compulsory membership in the party for trade unionists diminishes in the face of continuing close collaboration between union and party leaders. Donald Blake's pioneering study of this collaboration twenty-five years ago for the period 1889-1900, unmentioned in Hentilä's study,

offers a more sensitive approach to the problem. Finally, it may be premature to claim that the party had endorsed reformism fully by 1914. The possibility of violent revolution was not distant during 1917–18, nor is it inconceivable to revolutionize society through parliamentary means.

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MAX ENGMAN and JERKER A. ERIKSSON. *Mannen i kolboxen: John Reed och Finland* [The Man in the Coalbox: John Reed and Finland]. Summary in English. (Svenska Litteratursällskapet i Finland, number 485.) Ekenas: Ekenäs Tryckeri Aktiebolag. 1979. Pp. 231.

John Reed was a gifted American journalist in the days before radio and television when persons sensitive to world events depended on the written word to communicate a sense of immediacy and significance. He was also a cultural hero in the bohemia of New York's Greenwich Village until American entry into World War I shattered that community into mutually hostile factions. Reed's eyewitness account of the Bolshevik revolution was published as *Ten Days that Shook the World*, and by December 1919 he was back in Moscow as a delegate from the Communist Labor party of the USA. Early in February 1920 he entered Finland illegally on the first stage of a return trip to the United States, carrying false identity papers, diamonds, currency, documents and photographs for the use of American Communists, letters to American friends from Russians and from left-wingers recently deported from the United States, and a copy of Lenin's enthusiastic introduction to *Ten Days*.

On March 13, 1920, Reed was arrested by Finnish customs officers, who found him hiding in the coal bunker of the Åbo (Turku)–Stockholm passenger steamer. He was jailed in the southwestern Finnish port city of Åbo until June 4, when he was sent to Helsingfors to catch the next day's ship to Estonia en route to Soviet Russia. A Finnish law court on April 26 had found him guilty of smuggling, confiscated the diamonds and illegal currency, and imposed a fine, which was paid. Why was he held another five weeks without legal cause? What were the intentions of the Finnish authorities, of the U.S. State Department, or, for that matter, of Reed's Finnish associates?

Max Engman and Jerker A. Eriksson, two well-established Finnish historians, have consulted a mass of previously unanalyzed primary documents from the Finnish law courts, customs service, security police, and foreign ministry, as well as the U.S. National Archives and an impressive array of private archives in Finland and the United States.

They collate their results, point by point, with the accounts by Reed's major biographers: Granville Hicks, Richard O'Connor and Dale L. Walker, Barbara Gelb and Robert A. Rosenstone, along with more fragmentary references. In addition, Engman and Eriksson place the Reed episode in the context of Finnish left-wing political life around 1920, especially the efforts of Sulo and Hella Wuolijoki to cultivate foreign journalists as a means of counteracting right-wing political hegemony in Finland after the civil war.

Although Reed was under indictment for criminal anarchy in the state of Illinois, the U.S. State Department made no great effort to seize his person in the absence of an extradition treaty. The Americans were glad to have him immobilized in jail, and their primary interest was in the documents that Reed was carrying, since these could yield evidence for antileftist prosecutions being conducted in the United States. The Finnish foreign ministry did transmit copies of the documents to their American colleagues. The Finnish security police hoped to extract from Reed information that could be used to implicate Finnish left-wing socialists, such as the Wuolijokis, but Reed remained firmly silent about those Finns who had assisted his travels. By late May Reed was of little use and an increasing embarrassment to the Finnish government. When he began to threaten a hunger strike, the police quickly arranged his release and deportation, with the cooperation of a reluctant Estonian consul.

Engman and Eriksson introduce no major new interpretations, but they correct errors and lay to rest some speculations to which other historians have had to resort in the absence of hard data. They also convey much of that strange atmosphere that pervaded the fringes of the Soviet sphere during these tense years. A five-page English-language summary covers all major points. Documentation is meticulous, and the lengthy, multilingual bibliography of printed sources is an interesting compendium on northeast European history for this period.

ROBERTA G. SELLECK
Harvard University Library

PETER-MICHAEL HAHN. *Struktur und Funktion des brandenburgischen Adels im 16. Jahrhundert*. (Historische und Pädagogische Studien, number 9.) Berlin: Colloquium Verlag. 1979. Pp. ix, 369.

HEINZ REIF. *Westfälischer Adel, 1770–1860: Vom Herrschaftsstand zur regionalen Elite*. (Kritische Studien zur Geschichtswissenschaft, number 35.) Göttingen: Vandenhoeck und Ruprecht. 1979. Pp. 711. DM 98.

These two books are major additions to the small but growing body of important work on the social history of German nobilities. Both are dissertations, the former written under Klaus Müller at Düsseldorf and the latter under Jürgen Kocka at Bielefeld and Eberhard Weis at Munich, and they indicate the increasingly high quality of current German historical research. Both are strongly analytical, present their specific findings within broad perspectives, and are influenced by recent methodological developments outside Germany. Peter-Michael Hahn and Heinz Reif model their work, for example, on Lawrence Stone's *Crisis of the Aristocracy* (1965), as they both undertake prosopographical studies and emphasize family history. In addition, both these historians buttress the interpretation of German nobilities as social elites with remarkable flexibility, resiliency, and staying power. Since the books differ in geographical focus, temporal coverage, and analytical scope, however, I shall discuss them individually.

Hahn examines the "structure and function" of the Brandenburg nobility from about 1470 to 1620, a period in which the Junker gained considerable wealth and enjoyed strong political influence in the Mark. Based on extensive use of printed sources and secondary literature rather than archival research, Hahn's study brings few major surprises or new discoveries. But it does develop a highly intelligent analysis of the economic, social, and political activities of the nobles: landholding, entrepreneurial agriculture, intensified exploitation of towns and peasants, educational and marital patterns, and especially officeholding. Hahn asks the right questions and musters precise, convincing evidence to support his generalizations. His careful prosopographical work enables him to demonstrate the existence of a "power elite" of some twenty-five families in Brandenburg. Especially impressive is Hahn's emphasis on the economic base from which these families extended their social and political power. Hohenzollern dependence on loans from the nobles guaranteed their active participation in decision making, so that governing was clearly a joint function of rulers and aristocrats in this period before the development of strong central and bureaucratic institutions in the Mark. This is an old story, perhaps, but it has never sounded newer or been as convincingly reconstructed as in this fine synthesis—the first to which scholars should now turn for a penetrating overview of Brandenburg society and polity before the Thirty Years' War.

Reif's study of the Westphalian nobility from the late eighteenth through the mid-nineteenth century is one of the most impressive achievements this reviewer has seen in the entire field of German social history. It rests on an awesome command of primary sources: state, ecclesiastical, and family pa-

pers, published and archival. As a scholar with very broad interests and knowledge, Reif always relates his specific, localized research to general historical processes. He examines almost every aspect of the lives of an important group of Westphalian nobles—those qualified for positions in the cathedral chapter at Münster—through a century of jolting political, economic, and social changes. Some twenty-five *stiftsfähige* families form the basis for the study, but their experiences are compared whenever possible with those of their local colleagues as well as outside (especially East Elbian) aristocracies. We follow their history before, during, and after the major crisis of the Napoleonic period, which involved a loss of autonomy first to the Kingdom of Westphalia and then to Prussia. Their political, economic, social, and psychological adjustments to this shock are examined step by step, and by the 1860s the old ruling families of the bishopric had weathered all storms. They recovered economic and political initiative, buttressed their social position, and remained a potent regional elite within Prussia.

Even more impressive than the thesis itself is the stunning manner in which Reif succeeds in writing "the collective biography of a social group as the history of society" (p. 18). True, there are flaws in this book: the author is at times too didactic in placing his research in its broader context; his writing is often too heavy, certainly for Anglo-American tastes but probably also for Germans; and his argumentation is sometimes so finely nuanced that its clarity is lost until it is summarized. These stylistic deficiencies excepted, however, this is the book in German social history most comparable in substance, method, and quality to Stone's classic study of the English peerage. I have already mentioned the author's prosopographical method and his comprehensive approach. Group dynamics are always examined in terms of families and individuals, and no historian of the family should neglect this impressive analysis. Not only does Reif combine quantitative with qualitative evidence; he also shows equal concern for the roles of all members of a family—heirs, younger sons, and women. As things turn out, there is less emphasis here on economic developments than on the history of mentalities and affective relationships. For Reif, the history of education involves not only formal institutional changes and their costs but also informal socialization and even character formation. His treatment of religion as a unified political, social, and emotional-intellectual experience is simply brilliant. The author is fortunate, of course, in his choice of such an articulate group as his Münster nobles (who count, after all, Annette von Droste-Hülshoff among their number), but his real success here lies in his quite profound feeling for the interaction between the public and private lives of his subjects. I could go on in

praise of this major achievement, just as I shall hope for its wide reception despite its length, tendency toward repetition, and rather too heavy style. Let me conclude, however, with my view that Reif is a historian of commanding historical perspective, keen analytical powers, and very sensitive human perception. His book is a masterpiece.

GERALD L. SOLIDAY
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LAWRENCE J. BAACK. *Christian Bernstorff and Prussia: Diplomacy and Reform Conservatism, 1818-1832*. New Brunswick, N.J.: Rutgers University Press. 1980. Pp. xiv, 379. \$25.00.

This study presents a much-needed and well-rounded account of the major facets of Prussian foreign policy during the Bernstorff era, focusing in particular on Bernstorff's role as foreign minister. By relying on the existing literature and by supplementing it with extensive archival research, Lawrence J. Baack leads us skillfully through the various crises in Prussian diplomacy, starting with Bernstorff's circumscribed position during the early conferences. Baack elaborates on the foreign minister's support of Prussian customs policies and emphasizes Bernstorff's anti-Austrian stance vis-à-vis the Eastern crisis; the author brings out Bernstorff's great contribution to the maintenance of peace during the 1830-31 upheavals and reveals how he was maneuvered out of office by Metternich. Moreover, Baack details Bernstorff's precarious relations with King Frederick William III and the Berlin court as well as the internal workings of the Prussian Foreign Office.

Baack limits his evaluation of Bernstorff as a person mostly to categorizing him in terms of his family tradition and conservative ideology. Bernstorff, the gentleman with a charming manner, does not emerge; more comparisons with his Danish experience would have helped. By reading between the lines, one discerns Bernstorff as a political pragmatist with considerable diplomatic versatility, but his rank as statesman remains uncertain; one wonders to what extent opportunism, aristocratic naiveté, and bad luck characterized his career.

Better editing could have eliminated many major and minor lapses, including the confusing account of the Bernstorff family (pp. 1-2), the description of Luxembourg as a "separate polity" (p. 191), and the not readily understandable references to an Austrian president (p. 237) and a duchy of Beulen (p. 211 and *passim*).

To be sure, much of what is told here has been known, but Baack successfully adduces new insights and comes up with a balanced and thought-pro-

voking study that should make a major contribution to our understanding of this decade of Prussian history. It is difficult to realize that thirty years before Bismarck's rise to power, Prussian diplomacy was directed by a former Danish foreign minister, who pursued a policy of "rational" objectives with a modicum of success.

ARNOLD H. PRICE
American Historical Association

PAUL R. SWEET. *Wilhelm von Humboldt: A Biography*. Volume 2, 1808-1835. Columbus: Ohio State University Press. 1980. Pp. x, 572. \$25.00.

The second volume of Paul R. Sweet's admirably comprehensive study of Wilhelm von Humboldt begins with Humboldt's departure from Rome in 1808 and traces his political, diplomatic, scholarly, and literary existence to its conclusion in 1835. Some interesting new archival sources are used—dispatches Humboldt wrote as Prussian envoy in Vienna, for instance, or his correspondence with Peter Stephen Du Ponceau on the languages of the American Indian, now in the library of the American Philosophical Society. But in general, Sweet's biography relies on printed materials, a vast and diverse literature, which the author seems to have mastered completely.

It is the outstanding characteristic of this volume, as it was of its predecessor, that nearly all aspects of Humboldt's life receive equal attention. His linguistic scholarship and his historical writings are analyzed with the same thoroughness and balanced judgment that distinguish the sections on Humboldt the educational reformer, the diplomat, and the minister whose somewhat inept championing of a Prussian constitution and of an increase in ministerial authority ended with his dismissal and the defeat of the remnants of the reform party in December 1819. It may be thought that the biography of such a multifaceted personality can do no less; but usually biographers selectively emphasize aspects that particularly interest them and that are crucial to their interpretation. Yet in view of the richness of the material, it is not surprising that this work, too, does not avoid all unevenness. More might have been said, for instance, about the thousands of sonnets Humboldt wrote in the last years of his life. The brief, convincing comments on these psychologically revealing poems in Sweet's concluding analysis of Humboldt's personality suggest how valuable an extended discussion might have been.

In my review of the first volume (*AHR*, 84 [1979]: 783), I thought it too early to predict whether a significantly different picture of Humboldt would emerge from the complete biography. It is now apparent that this was never the author's intention.

Although Sweet assembles and integrates much dispersed information, places highlights differently, and offers many new insights, our general impression of Humboldt's life is hardly changed. But to say that is to characterize, not to criticize, the work. Both as a biography and as a study of German politics and culture, Sweet's *Humboldt* is a genuine and welcome achievement.

PETER PARET
Stanford University

ROLAND KIRCHHERR. *Die Verfassung des Fürstentums Hohenzollern-Sigmaringen vom Jahre 1833: Zu den Auswirkungen der Verfassungstheorien der Zeit des Deutschen Bundes auf das Fürstentum Hohenzollern-Sigmaringen.* (Dissertationen zur Neueren Geschichte, number 5.) Cologne: Böhlau Verlag. 1979. Pp. v, 447. DM 74.

To trace the evolution of a small South German state's constitution, to observe the influence on this process of prevailing liberal and constitutional theory, and to ascertain whether the favorable reception granted the Hohenzollern-Sigmaringen constitution among liberal-minded contemporaries was justified—these are Roland Kirchherr's aims.

Like the constitutions of neighboring states, the Sigmaringen document reflected a determination to combine the monarchical principle and estate corporatism with the fundamentals of parliamentary rule. And it evinced a surprising familiarity with current juridical arcana. Although the constitution was not formally imposed (*oktroiert*), this was owing less to a gesture of monarchical beneficence than to government shrewdness in recognizing that imposed constitutions invited popular resistance. Executive power still resided with the king, as did rights of legislative initiative and veto. Prince Charles, advocate of constitutionalism and successor to the throne in October 1831, left no doubt that there were definite limits to his liberality.

Other areas of constitutional theory covered in this needlessly pedestrian catalogue are the rights and duties of the citizen, church-state relations, ministerial responsibility, the achievement of a balance between the local quest for self-administration and princely prerogative (the communities were given considerably leeway in managing their own affairs), the organization of the *Landtag* (Sigmaringen opted for a single chamber, deemed more appropriate for a state in which the number of elected representatives in 1833 was only twenty-three, or roughly one for every 3,000 subjects), electoral procedure (indirect voting with age and property qualifications), the meaning of representation, and the determination of a method for facilitating constitutional revisions.

Constitutional history has matured into a highly

challenging field demanding familiarity with the analytical tools of several disciplines. The work under review gives no hint of any such advance. Certainly, given the goals Kirchherr set for himself, it is a nice, diligently researched contribution to Vormärz political history. But political history this narrowly conceived gives us a good deal less than half the story. Prince Charles and his advisers had no illusions regarding the prophylactic value of a constitution in protecting the crown against various strains of liberalism. Although elected representatives were given an opportunity to debate the clauses of the constitution, they were quite powerless and had perforce to rest content with proposals advanced by the crown. One is tempted to suggest that government charitableness might have been placed in rather a different light had Kirchherr probed deeper into government motives by, for example, investigating state finances for the post-1815 period (Herbert Obenaus) or examining the severity of the 1830 crisis in Hohenzollern-Sigmaringen (Heinrich Volkmann).

A study of this kind broaches a more fundamental issue. Kirchherr concludes that through its provisions for juridical security, the constitution was an unmistakable sign of political progress. This is quite so. But elitist adaptation in pre-industrial society meant paper guarantees affecting the few rather than the many. In 1834 an economically disadvantaged subject in Sigmaringen could presumably congratulate himself that he was now an economically disadvantaged citizen and applaud the *de jure* transition from patrimonial rule to representative government. Moreover, within the German cameralist tradition, monarchical caprice was viewed as an anathema, constitution or no. Political history rooted in little more than documentary analysis invariably distorts processes of modernizing change and will therefore arouse little interest. Unfortunately for Kirchherr, this applies in his case.

KARL H. WEGERT
University of Saskatchewan

DAVID GROSS. *The Writer and Society: Heinrich Mann and Literary Politics in Germany, 1890-1940.* Atlantic Highlands, N.J.: Humanities Press. 1980. Pp. xvi, 302. \$15.00.

Heinrich Mann (1871-1950) was one of the intellectual leaders of the German empire and the Weimar Republic. Before the First World War he led the way from aestheticism to democratic humanism, and afterward he was actively engaged in politics as a dignitary of the republic and a leader of the antifascist exile. His opposition to Nazism made this senator of the spirit a prominent fellow traveler

and, after the Second World War, one of the few symbols of legitimacy for the DDR.

David Gross has written a straightforward and orderly account of Heinrich Mann's development as a political and social critic. He concentrates on two related issues: the evolution of Mann's conception of the writer from *Lyriker* to *Literat* and Mann's evolution between 1890 and 1940 from conservative to liberal to radical. Gross also treats Mann as a socioliterary type of his generation and contributes to our understanding of German writers born in the 1870s and 1880s. He argues that an intellectual biography of Mann "reads much like the biography of the era itself" (p. viii), but Gross's attempt at writing a sociology of literature is often too abstract and indifferent to the concrete reality of Mann's life. At times Mann disappears under an excess of analysis, and questions of motivation and influence are not dealt with persuasively. Moreover, Gross makes no attempt to define the contribution of his work in relation to the other scholarship on Mann or twentieth-century German thought, and his argument is marred by such distractions as his persistent misuse of the word "infer."

Gross offers a divided interpretation that emphasizes both the "epistemological break" in the years before 1910 and the ultimate unity of Mann's development. This tension corresponds to the conflict between Gross's admiration for Mann's decision to politicize his writing and move toward democratic humanism and his discomfort with the fact that Mann remained in the end only a writer—and one who was committed to a middle-class metaphysics of *Geist* that prevented him from becoming a revolutionary writer.

The most original aspect of Gross's portrait is his description of Mann's reactionary views in the 1890s. It is sobering to learn that even this apostle of reason began as an opponent of liberalism, socialism, and Jews. As the editor of the conservative *Das Zwanzigste Jahrhundert*, the young Mann advocated monarchism, nationalism, elitism, and war. Writing the novels of 1900 to 1909 led Mann to a fundamental transformation of his world view, and Gross finds signs of this metamorphosis as early as 1895. But the nonpolitical aspects of Mann's mind hold little interest for Gross, who has only praise for Mann's sacrifice of literature to politics.

Nonetheless, Gross argues that Mann's outmoded attachment to *Geist* kept him from understanding Weimar politics and society, and Gross is critical of Mann's actual political judgments and his unrealistic notion of the power of intellectuals. Gross emphasizes Heinrich Mann's role as "a path-breaker in his conception of the role and function of the literary intellectual in Germany" (p. 96), but the effect of this book is to recall the tendency of German in-

tellectuals to preach at and be ignored by the average German.

DAVID S. LUFT
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San Diego

ANNA WOLFF-POWĘSKA. *Doktryna geopolityki w Niemczech* [The Doctrine of Geopolitics in Germany]. Summary in English. (Studium Niemcoznawcze Instytutu Zachodniego, number 34.) Poznań: Instytut Zachodni. 1979. Pp. 303. 75 Zł.

This is a survey of geopolitical ideas and influence in Germany from the Romantic era to 1945. It is designed to meet an apparent void in the Polish literature and to appeal more to a general audience than to specialists. Anna Wolff-Powęska has conducted a thorough and wide-ranging examination of the mostly German literature on her subject. Indeed, for long stretches this work has the character of a bibliographical essay, which may be its chief value to Western scholars, whether or not they read Polish.

On the other hand, her treatment of German geopolitics is generally nonanalytical and yields few new insights. Her concept of geopolitics is extremely diffuse and includes almost any formulation of political objectives in terms of "large-space" categories. Almost half of this work consists of a general survey of supposed geopolitical antecedents. But in the absence of a reasonably tight definition of the subject itself, the result is a rather hackneyed treatment of the development of German political nationalism, which seems virtually indistinguishable from Wolff-Powęska's notion of geopolitics. Any suggestion of a geographical rationalization for a nationalist goal suffices to put one in the geopolitical camp. Burke, Romantic "mystics" in general, Clausewitz, Adam Müller, Hegel, List, Roschau (*sic*), Bruck, Droysen, Rössler, Frantz, Lagarde, and even Nietzsche are to be found here among early contributors to the doctrine. Curiously, the more pertinent role in the geopolitical pedigree of efforts to extend the scientific method into areas where it does not apply, such as eighteenth-century rationalism and nineteenth-century positivism, is ignored.

The second half of the book, focusing on the interwar years and based occasionally on primary sources, including the Haushofer family archives, is somewhat better. The historical role of geopolitics culminated, of course, under National Socialism: Rudolf Hess, a student of Karl Haushofer at Munich University, provides the chief personal link between geopolitics and Nazism, although Wolff-Powęska points out, with reference to the rocky relations after 1933, that the two doctrines were not exactly

coterminous. She focuses mainly on the spread of geopolitical terms and thinking under Nazism, not on the doctrine itself. She makes little effort to examine the theoretical presuppositions or the arguments used to support the many (and varied) geopolitical contentions. She devotes disappointingly little space to the actual geopolitical thinkers: Haushofer and his direct predecessors, Rudolf Kjellén (who coined the term) and Friedrich Ratzel (whom Wolff-Powęska credits with being the pioneer of "large-space" theorizing). The decision to restrict coverage to German geopolitics is unfortunate, for it both omits a number of interesting non-German figures (among Poles, Roman Dmowski is the first to come to mind) and obscures the question of just how "German" this idea actually was. The basic thesis of the work is that, for the most part, geopolitics can be reduced to an effort to rationalize pre-existing national-political goals, especially during the Nazi era.

In sum, this is a solid research effort, comprehensive almost to a fault, but also one that is exclusively narrative, conceptually weak, and contains little that should cause the Western specialist to secure a translation.

RICHARD BLANKE
University of Maine,
Orono

DAVID CLAY LARGE. *The Politics of Law and Order: A History of the Bavarian Einwohnerwehr, 1918-1921*. (Transactions of the American Philosophical Society, number 70, part 2.) Philadelphia: The Society. 1980. Pp. 87. \$8.00.

In an informative book of manageable proportions, David Clay Large convincingly argues that as "a factor in postwar German politics . . . the [paramilitary] Einwohnerwehr movement deserves more attention than it usually gets" (p. 77). Following an effective introduction on the impact of war and revolution, Large devotes most of the book to a discussion of the phases of *Einwohnerwehr* development in Bavaria and then of its short-lived expansion in northern Germany and in Austria. He emphasizes the shifts from public to private control, from multiclass to rightist make-up (although he is not entirely clear whether it attracted mainly conservative middle classes [p. 31] or peasantry [p. 34]), and from localized to centralized organization, with a growing role for mobile detachments of freebooters. Large is careful to limit his study to the *Einwohnerwehr* led by Dr. Georg Escherich and Rudolf Kanzler and their *Orgesch** and *Orka* extensions, and he thus does not get drawn into digressions on Bavaria's other right-wing paramilitary factions.

Throughout he analyzes Escherich's relationship first with the socialist Hoffmann and then with the conservative Kahr governments.

Large concludes his book with an assessment of the *Einwohnerwehr*'s contribution to the failure of the Weimar Republic and to the rise of Hitler. He rejects the contention that the *Einwohnerwehr* was traditionally or safely conservative, thus denying that it would have blocked the rise of the Nazis had the victorious governments not forced its dissolution in June 1921. Large notes that, however much the dissolution may have hastened the growth of more radical and *völkisch* groups of alienated opponents of the postwar order, the *Einwohnerwehr* was already losing young hotheads to other organizations. He also contends that Escherich and the *Einwohnerwehr* leadership paved the way for the Nazis by making "respectable" the goal of replacing traditional state authority "with a private law of its own design" justified by the claim that the *Einwohnerwehr* held a higher authority based on "the spontaneous will of the citizens" (pp. 78-79). Thus, the *Einwohnerwehr* and its northern German and Austrian extensions, which briefly embraced an "empire" of nearly two million men, contributed significantly to the radicalization of rightist politics throughout Central Europe well before Hitler became prominent. Typically, it was largely Escherich's intransigent refusal to make cosmetic compromises to mollify the Allies that brought about the organization's dissolution.

Large bases each step of his work on impressive archival research, rivaling that of Ludger Rape in his lengthy study of the relations between the Austrian *Heimwehr* and the Bavarian right. One might criticize some stylistic unevenness (Kahr or von Kahr?) and occasional emphasis on insignificant detail. But in the main one must compliment Large on his deft and succinct treatment of his subject.

C. EARL EDMONDSON
Davidson College

PETER BRANDT and REINHARD RÜRUP, editors. *Arbeiter-, Soldaten- und Volksräte in Baden, 1918/19*. (Quellen zur Geschichte der Rätebewegung in Deutschland, 1918/19, number 3.) Düsseldorf: Droste Verlag. 1980. Pp. cxxii, 504. DM 198.

Like the other volumes in its series—on the Berlin *Zentralrat* and the workers' and soldiers' councils of Württemberg—this documentary volume is assembled, edited, and annotated with resourcefulness and skill. Hardly any of the internal documentation of the Baden councils, such as minutes of meetings, has survived, so that what Peter Brandt and Reinhard Rürup provide is a cleverly constructed tissue of decrees and reports extracted from rare newspa-

pers, handbills, and pamphlets, together with the scrappy remains of correspondence and the like dug out of nearly twenty archives. The number of scholars who will want to work extensively in these documents must be, as the Germans say, vanishingly small. Others will want to consult the volume on particular subjects, the search for which is facilitated by an excellent index. But if it is not altogether clear for whom the job was done, it was done well.

Even for most specialists, the main interests of such a volume lie in its scholarly preface, here over 100 pages long. For the most part, it is a contribution to the now voluminous literature on the workers' and soldiers' councils, seen under the aspect of "the aims and dynamics of the mass movements that spontaneously arose in November" of 1918 (p. cxiv). Baden has not featured largely in previous analyses; and with its relatively rural economy and relatively advanced political life, its particularism, and the unusual orientation of its socialists, the *Musterlande* has some special traits to display.

The analysis cannot be said to break new ground, although it is conducted at the high level of sophistication made possible by the progress of such studies in Germany. If there are deficiencies, they come from the grievous gaps in the sources: the lost files of Baden's Ministry of the Interior or the sparseness of information on council radicalism in the state's principal industrial city, Mannheim. Of more general interest is an extended analysis of the relevant social and political characteristics of Baden before 1914. And there are nuggets. The pages on the impulses to revolution in Baden (pp. lii-lv) are unusually suggestive. Those that sum up the nature of Baden's revolution (pp. cxiv-cxv) are magisterial, in the best manner of the senior editor, Rürup, one of the most thoughtful and original commentators on the German revolution. These passages will repay the attention of any specialist in the period.

DAVID W. MORGAN
Wesleyan University

HANS ROBINSOHN. *Justiz als politische Verfolgung: Die Rechtsprechung in "Rassenschandefällen" beim Landgericht Hamburg, 1936-1943.* (Schriftenreihe der Vierteljahrshefte für Zeitgeschichte, number 35.) Stuttgart: Deutsche Verlags-Anstalt. 1977. Pp. 167. DM 19.80.

Especially perfidious among the obscenities generated by Hitlerism was the mobilization of the judicial system to prosecute Jews charged with having engaged in extramarital sexual relations with Germans of "aryan" ancestry, or vice versa, contrary to the provisions of the "Law for the Protection of German Blood and German Honor," the second of

the so-called Nuremberg racial laws of September 1935. In pursuit of the politicization of justice to achieve Nazi ideological goals (in particular the elimination of all elements deemed inimical to Germanism's survival) and supposedly in accordance with a "healthy popular sense" of right and wrong, the provincial court in Hamburg—chosen by Hans Robinsohn as representative in the tenor of its judgments although evidently much readier than other jurisdictions to bring such "offenders" to trial—arraigned some 430 males accused of "racial defilement" between 1936 and 1943.

Utilizing police and court transcripts and also postwar restitution records, Robinsohn examines the brutal Gestapo interrogation methods, the rigorous determination by the judges of the crucially important racial categorization of the defendants, as well as whether or not illicit intercourse (broadly defined by the German Supreme Court as "the satisfaction of the sexual urgings of at least one of the partners") had taken place, and the dubious activity of the often prejudiced "experts" (*Sachverständige*) appointed to advise the bench, which taken together resulted in convictions in 91 percent of the cases. Sentences passed ranged, on the average, from 500 days in jail to prison terms of several years, with those against Jews both more frequent and more severe than those against Germans. The numerous verdicts quoted verbatim or summarized at length make clear the frightful arbitrariness, inhumanity, and essential illegality of the proceedings; even elderly Jews were incarcerated for continuing marriage-like relationships that long predated 1933 and were afterward deported to death camps.

As for the "central problem of Nazi jurisprudence" (p. 123), the independence of the courts in reaching their decisions, the author concludes that, despite systematic pressure from superiors, judges and state attorneys could indeed soften the application of the law. That this occurred so seldom in Hamburg, but instead that those responsible readily helped pervert justice, he attributes rather vaguely to their authoritarian, antisemitic, and nationalistic bourgeois backgrounds. The same legal positivism in the service of "reason of state" that they displayed during the Third Reich, however, ensured their rehabilitation, for the most part, after 1945—yet another continuity in German history?

LAWRENCE D. STOKES
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ANTHONY KOMJATHY and REBECCA STOCKWELL. *German Minorities and the Third Reich: Ethnic Germans of East Central Europe between the Wars.* New York: Holmes and Meier. 1980. Pp. xii, 217. \$26.50.

Although it bears the marks of Anthony Komjathy and Rebecca Stockwell's inexperience and limited knowledge of National Socialist Germany, *German Minorities and the Third Reich* should prove a useful book. It brings together in a small compass a wealth of information about the ethnic Germans of Bohemia (not Slovakia), Poland, Hungary, Rumania, and Yugoslavia during the period 1919–41. It also offers some insight into these peoples' dealings with Weimar and Nazi authorities and presents a balance sheet of the respective loyalties of each folk group (*Volksgruppe*) to the states of which they were citizens. The authors conclude that even the Weimar Republic pursued revisionist ends in supporting the ethnic Germans in Eastern Europe; the National Socialists, however, jettisoned a tradition of genuine concern for the well-being of the *Volksdeutschen* while using the divisions and frustrations of those groups to pursue imperialist aims. By and large the authors excuse the ethnic Germans' cooperation with Hitler because (1) large majorities were either inert or actively loyal; (2) desire to preserve cultural autonomy in the face of systematic policies of assimilation thrust old-line traditionalist leadership into dependence on German governmental handouts even before 1933; and (3) the depression gave rise to a generation gap whereby at least some of the younger people turned to the Nazis out of economic and social desperation.

Komjathy and Stockwell use the State Department's *Documents on German Foreign Policy* (Series C and D) carefully and fully. They also use many VDA (*Verein für das Deutschtum im Ausland*) records as well as interviews they gathered themselves plus those deposited with the *Bundesarchiv* in Koblenz. The latter, incidentally, seem quite apologetic, because sufficient care is not always exercised in their use. Additional valuable material comes from the *Politisches Archiv* of the German Foreign Office and from a published collection from Hungary, Magyar Tudományos Akadémia Történettudományi Intézete, *A Wilhelmstrasse és Magyarország: Német Diplomáciai Iratok, 1933–1944* (1968). The authors also tap many other primary sources and are largely up to date in referring to recent scholarly works in the ethnic German field. Recent material from Hungary is especially well represented. An unfortunate omission because of its greater comprehension of the National Socialist context is Ronald M. Smelser, *The Sudeten Problem, 1933–1938: Volkstumspolitik and the Formulation of Nazi Foreign Policy* (1975).

This is an uneven book, in which the treatment of Hungary and Rumania is the most thorough and novel. The authors seem to have it in for the ethnic Germans of Yugoslavia, who seem to be subjected to more rigorous standards than the rest and are found wanting. Both the Czechoslovak and the Polish ethnic Germans have been handled better else-

where. The subtitle, *Ethnic Germans of East Central Europe between the Wars*, promises too much; 90 percent of the treatment refers to the period after 1933.

ROBERT KOEHL
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SAMUEL HUGGEL. *Die Einschlagsbewegung in der Basler Landschaft: Gründe und Folgen der wichtigsten agrarischen Neuerung im Ancien Régime*. In two volumes. Summary in French and English. (Quellen und Forschungen zur Geschichte und Landeskunde des Kantons Baselland, number 17.) Liestal: Kantonale Schul- und Büromaterialverwaltung. 1979. Pp. xxi, 838.

Einschlagsbewegung may be translated as "enclosure"; specifically, however, it refers to that process by which the authorities of Basel, during the second half of the eighteenth century, allowed their rural subjects to petition to liberate specific and already privately owned acreages and properties from communal restrictions and regulations governing land use, such as compulsory crop rotation and open common pasturage after harvest, and from such "feudal" obligations as tithe and labor services. This study's strongest aspect is Samuel Huggel's careful tracing of the complicated institutional and economic "ecology" of effects and countereffects that accompanied *Einschlag*. Huggel's somewhat whiggish general conclusion that enclosure helped "the man from the countryside . . . to a better life" and that "that was all that mattered" (p. 522) is contradicted by much of the evidence.

Enclosure permitted a pluralization of the rural economy on an unprecedented scale. Increases in grazing lands, conversion to dairy farming and to viticulture along the lower mountain reaches all diversified agriculture so that it could support a growing rural cottage industry in silk ribbon weaving. Divergent environments, economies, and techniques intermeshed more closely and efficiently, and the productivity of the region increased absolutely. The rich detail Huggel provides, his eye for connections, and his quantitative evaluations of a diverse collection of primary materials are impressive, but occasionally the numerous pages of equations and symbols make for difficult reading.

The quantitative analyses do not probe deeply into the social changes related to enclosure. For example, concerning rural indebtedness, the author expresses puzzlement that a rural population whose indebtedness was increasing overall could also increase its share of the credit market. Had he combined an analysis of the stratification of indebtedness and bankruptcy with his earlier observations that some wealthy peasants speculated

successfully with enclosure (given the tacit support of the authorities) and were able to reduce their debt while many smallholders, hoping to improve their limited economies through enclosure, failed to profit, then this apparent incongruence would not have arisen. Instead of a thoroughly conceptualized social analysis, the last part of the book presents a superficial treatment of several fashionable themes: education, town and country conflicts, the downward valuation of older males and the upward valuation of women, *Mentalität*, family structure, and so forth. Enclosure allegedly reinforced an already-existing trend away from corporatism and toward individualism, but this contradicts an earlier observation that new kinds of corporations, namely, consortia of wealthy peasants, were effectively cornering the speculative land market that resulted from enclosure. Conflicts are presented as conflicts between individuals in a pluralizing society when the evidence clearly points to conflicts between groups of winners and losers. Huggel hints at but does not develop such subtler themes as status discrepancy (experienced by rural clergy and industrial workers) and party formation.

Books about the European experience with the "commons" have a special relevance for the present because we are still grappling with historically unresolved questions about the proper relationship between public and private areas of human life. The core of any such discussion must be social relations. As one of Garrett Hardin's critics has observed, the liberal economic flight from the tragedy of the commons (of which Huggel describes one instance) leads only to the comedy of community, masking class conflict. It is this result of liberal rural reform that is ignored by this book. Nevertheless, this is in many ways an excellent and significant work whose rich data, numerous tables, and detailed economic analyses provide a strong basis for further comparative history.

HERMANN REBEL
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T. S. R. BOASE. *Giorgio Vasari: The Man and the Book*. (A. W. Mellon Lectures in the Fine Arts, Bollingen Series XXXV, number 20.) Princeton: Princeton University Press, for the National Gallery of Art, Washington. 1979. Pp. xxiv, 367. \$25.00.

T. S. R. Boase's Mellon Lectures (1971) have been turned into a fine book on the founder of art history. Vasari, one feels, would have appreciated this tribute. He has been discussed, pro and con, almost as much as those who lived the *Lives* themselves, yet seldom with the respect and affection that the late Professor Boase displays. This is not a biography that makes strong new contributions to Vasari

scholarship. But it does portray Vasari as he has not been portrayed before: first as a man of his time; then, in successive chapters, as an author, as a pleasantly xenophobic critic with all sorts of fancies about the *maniera greca*, the *maniera tedesca*, and so forth. We also learn about Vasari's favorite cities—Assisi, Pisa, Florence, and above all his native Arezzo. We are diverted in the labyrinths of his chronological howlers and by his confluences of personalities. Boase presents us with a map of Vasari's misreadings that leads us directly into the mind of one of the most modern (because so insecure) and sympathetic (because the insecurity is so intimately proclaimed) figures of the Renaissance. Vasari gives heart to those who believe that the past, in Vico's phrase, is "a serious poem."

There is also a chapter on Raphael and Michelangelo, two of the three artists who gave its greatness to Renaissance art yet who, through that greatness, forced Vasari and all his generation into a belatedness that has made Vasari the chronicler of their deeds better known than Vasari the painter and architect. The book, which is excellently and copiously illustrated, ends with chapters on Vasari's views of his other contemporaries and on his last years.

Boase's book presents Vasari as an author, describing the uses he made in his own book of other books—for example, the *Libro di Antonio Billi* and Ghiberti's *Commentarii*—and the men, above all Paolo Giovio, who helped him conceive and write his own great work. The Princeton University Press's blessed ability—unusual nowadays—to unite illustrations, captions, text, and notes page by page means that the reader gets good value for his money and his time.

What does he not get? For one thing, no picture of how Vasari fits into the stream of art historiography and criticism, of how he bridges the gap between Cennino Cennini and Lomazzo. And curious misconceptions result from Boase's rather Vasarian lack of rigor. Thus on one occasion Vasari's penchant for emblematic wit is not illustrated as it should be. The Latin motto that appears on the title page of the 1568 edition of the *Vite* is: HAC SOSPITE NUNQUAM HOS PERIISSE VIROS, VICTOS AUT MORTE FATEBOR, which Boase translates without comment as, "I shall claim that with this breath these men have never perished, nor been conquered by death." Vasari's phrase is an adaptation of *Aeneid* VIII, 470–1, but with *hac* substituted for Virgil's *quo*. Evander, king of Arcadia, is telling Aeneas that while he, Aeneas, lives, Evander cannot accept the fact that Troy and its kingdom have fallen. By substituting *hac* for *quo* Vasari is making the subject of the verb "lives" feminine.

Here we must stop thinking of the words alone and look at their visual context, the title page

(which Boase reproduces). Its design consists of a cartouche formed of brushes, chisels, squares, and other implements of design. Hence we can decide that the antecedent of *hac* is *ars*. While art lives, Vasari emends, he refuses to accept the idea that these men have passed on, that death triumphs over them. The central illustration within the cartouche is borrowed from Signorelli's Orvieto *Last Judgment*: the buried bodies of Vasari's artists are resurrecting themselves, painfully crawling out of the earth at the summons of Fame—that is to say, of course, Vasari's trumpet voice.

The 1568 title page of Vasari's *Vite* means, then, that as Aeneas led Troy's remnants to Rome, there to found a greater, unfallen kingdom, so Art leads this band of brothers from death through resurrection to eternal fame in the Heaven of Vasari's book. Vasari sets himself the mighty precedent of Rome's great poem and escapes his own belatedness as an artist. The irony is that this very belatedness was laid upon him by the heroic stature of his predecessors, a stature he himself did so much to enhance.

GEORGE L. HERSEY
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GIOVANNI VIGO. *Fisco e società nella Lombardia del Cinquecento*. (Annales Cisalpines d'Histoire Sociale, series 2, number 3.) Bologna: Società Editrice il Mulino. 1979. Pp. 336. L. 10,000.

In the middle of the sixteenth century, as a result of the increased fiscal pressure exerted on Lombardy by its recent incorporation into the Habsburg empire, a new and extensive patrimonial survey (*estimo*) was undertaken, on the basis of which new levies might be assessed. The preparation of this survey, the disputes to which it gave rise, and the political frictions that it generated lasted for nearly half a century. Divisions within the Lombard state arose as larger cities, principally Milan, found their interests pitted against those of smaller ones; urban interests clashed with those of rural ones; merchants came into conflict with artisans and laborers, as each group sought to protect its own collective interests by diminishing the fiscal burden imposed on its members. In the course of these disputes, perhaps the greatest and most heated issue was the question of whether merchants' liquid assets would also be subject to taxation, and if so on what basis assessments of merchandise would be reached.

Giovanni Vigo's latest book, despite its somewhat ambiguous title, addresses itself to the disputes that the proposed patrimonial survey generated in Lombardy during the second half of the sixteenth century. The relationship that he seeks to delineate between "fiscality and society" includes hardly any of those issues that David Herlihy's and Christiane

Klapisch's rather more modestly titled but much more substantial analysis of the fifteenth-century Tuscan situation presents. Vigo's principal aim is to chronicle, often in minute and tedious detail, the battle of memoranda between representatives of the various Lombard collectivities, as each sought to defend itself before the imperial authorities. The outcome of these exchanges was the preparation of a patrimonial survey that survived, according to Vigo, until the mid-eighteenth century. The final distribution of the fiscal load reflected the economic and political preponderance of Milan, particularly of its merchants, as they succeeded in ensuring for themselves a more lenient treatment than their substantial wealth might have warranted. Furthermore, according to the author, the imperial government, concerned to administer its Lombard province equitably, often favored the rectification of old injustices, previously tolerated by the ducal and communal governments, which shifted an enormously disproportionate burden of taxation onto the inhabitants of the countryside.

The story that one finds in the pages of Vigo's book contains few surprises, although a number of points incidental to the main argument are developed interestingly and clearly. The principal shortcoming of the book, which seriously compromises its value, results from the author's extremely heavy reliance on the memoranda exchanged in the course of this protracted dispute. Basic statistical questions, such as the burden of taxation and the fiscal demands made on its Lombard province by the empire are not even explicitly asked, much less systematically answered. Nor is there any attempt to analyze the massive amount of documentation that the compilers of the Lombard *estimo* prepared during the second half of the sixteenth century, a task that Vigo promises to perform in the future. Thus, in the end, it appears to this reviewer that the book under examination here might serve as a somewhat too long and prolix introduction to a work that the author intends to write.

ANTHONY MOLHO
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FRANCO VENTURI. *Settecento riformatore*. Volume 3, *La prima crisi dell'Antico Regime, 1768-1776*. (Biblioteca di Cultura Storica, number 103.) Turin: Giulio Einaudi Editore. 1979. Pp. xviii, 458. L. 25,000.

This is the third volume of an ambitious project that puts the eighteenth-century Italian Enlightenment and reformers into their European context. It focuses on a particularly eventful nine-year period, 1768-76. Franco Venturi exemplifies the period in the words of Voltaire, written in 1772 to D'Alembert: "Doesn't this century seem to you to

be one of revolutions, starting with the Jesuits, and ending with the Swedes, or perhaps not ending at all?" (p. xii). The central focus is a series of political and economic crises, such as the revolts of Corsica, Greece, and Sweden, East European peasant rebellions, and food protest in Central and Western Europe. These roughly contemporaneous crises are seen as at base a consequence of fiscal overextension due to war and as a prelude to the democratic revolutions at the end of the century. Neither origins nor consequences are examined in any detail, however, nor are the crises themselves. Rather each crisis, organized in terms of geographic or political units, is examined first through a review of recent scholarship and second as it was perceived by contemporary Italians who wrote or read about it.

The view from Italy provides by far the most interesting passages, as do some interpretive insights, unfortunately somewhat scattered through the text. Italian, and especially Tuscan, gazettes are the major source of Italian opinion for Venturi. Of course, these publications were written for literate and sophisticated audiences. It is fascinating for the present-day reader to see the news of Pugachev's rebellion in the words through which a frequenter of Florentine cafes got his first inkling of events in Russia. The "Notizie del mondo" of February 12, 1774 that carried this report also brought the first word of the Boston Tea Party. Venturi also examines Italian opinion through the biography and letters of some less well-known intellectuals and influential journalists. One of these was Giovanni del Turco, a Florentine librarian, adventurer, and sometime publisher, who was responsible for the translation into Italian of Catherine the Great's "Instructions." Alessandro Verri's letters provide frequent illustrations—for example, his London letters of 1766–67, full of admiration for the freedom of everyday life and expression that he was observing. Venturi does not let Verri's naive enthusiasm pass uncriticized, of course. He notes Verri's lack of analysis of political and economic context; nevertheless, Verri's opinions offer an interesting counterpoint to the constitutional challenges of Wilkes and his supporters and of the North American colonists—the crises that that chapter treats. The book closes with a passage from the Declaration of Independence as published in Florence on September 3, 1776.

Because this study is organized chronologically and by political units, some "crises" of similar types must be searched out in widely separated passages of text. The problem of freeing the grain trade, for example, is treated separately in the chapters on Russia and France; this is one topic where comparative interpretation could usefully have been more fully developed in a single section. Venturi's broad perspective leads him to note perceptively (p. 95) that the program of intellectuals who called for

liberty of commerce in grain was made possible by the massive entry of Russian cereal crops into international trade as a consequence of Catherine's liberal policy, but this is not elaborated. Nevertheless, Venturi's work is a scholarly and elegant contribution to our understanding of the eighteenth-century community of knowledge in Europe and a lively and interesting interpretation of political events.

LOUISE A. TILLY

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RUDOLPH M. BELL. *Fate and Honor, Family and Village: Demographic and Cultural Change in Rural Italy Since 1800*. Chicago: University of Chicago Press. 1979. Pp. xiii, 269. \$22.00.

Rudolph M. Bell has written a comparative study of four Italian rural communities: Albareto in the Emilian Appennines, Castel S. Giorgio in Campania, Rogliano in Calabria, and Nissoria in Sicily. The communities he has chosen enable him to compare villages with small towns, and dispersed settlement with nuclear. He finds a contrast between the "self-contained world" of the villages of Albareto and Nissoria, where peasant culture retained some autonomy, and the towns of Castel S. Giorgio and Rogliano, where peasants failed to maintain their self-respect in the face of the contemptuous attitudes of local townspeople.

Bell's main concern is with traditional culture, which he sees as governed by the four values of his title. One of the main characteristics of peasant culture, he argues, was a cyclical view of time. In his explanation of change Bell leans heavily on the seasonal patterns of birth and death. The ability to control and alter these patterns is seen as the major reason for the adoption of a more modern viewpoint. This is not altogether convincing. Bell does not discuss the ways in which villagers actually reckoned time. When did the idea of an hourly wage take hold? And how many returned emigrants had watches? Bell might have done better to discuss space and time together as interdependent categories. The change in the peasants' concept of time could then have been related more clearly to the changing relationships of the village with the outside world.

There is a general problem about the type of evidence Bell uses and the type of conclusions he wishes to draw. His handling of the demographic evidence is both sophisticated and imaginative, but there are times when one feels that he stretches it too far in his attempts to infer peasant mentality. Without more precise references, it is often difficult to judge how far his interview material supports his conclusions. On the rare occasions when Bell goes beyond his chosen themes and speculates about pol-

itics and the wider world, this difficulty is particularly evident. The assertion that the Communist party owes its appeal to the peasant belief in destiny is interesting but unsupported by evidence. As for his explanation of Fascism as an outgrowth of peasant *campanilismo*, I find it decidedly convincing.

Bell is on much stronger ground when it comes to the family. He rejects Banfield's concept of "amoral familism," and finds that southern peasants organized their lives in terms of "a pattern of reciprocal obligations" among extended kinship networks. His discussion of the stages by which children won a measure of control over the choice of their marriage partners is one of the sections of the book in which anthropological and demographic evidence are most successfully fused. His chapter on emigration is also successful in getting behind the figures and showing the variety of reasons that contributed to the decision to emigrate.

This is an intelligent study that asks many new and interesting questions. Historians should be grateful to Rudolph Bell for posing them, even if they may not always be convinced by his answers.

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MARIA LUISA CICALESSE. *Note per un profilo di Pasquale Villari*. (Studi di Storia Moderna e Contemporanea, number 7.) Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1979. Pp. 248. L. 10,000.

Pasquale Villari seems a natural subject for biography. He was a serious and broad-ranging intellectual whose life and work combined many of the preoccupations of nineteenth-century Italians. A study of his life could, therefore, also be a window on his times, an approach to biography that Villari himself emphatically supported.

A Neapolitan forced into exile in 1848, Villari made the rest of his academic career in Pisa and Florence, where he became the leading biographer of two Florentine heroes, Savonarola and Machiavelli. These biographies show the clear influence of the concerns of his own day—neo-guelfism, unification, reform—that, in some ways, limited his understanding of the sixteenth century. On the other hand, his serious intellectual commitments, combined with sound scholarship and a feeling for detail, helped to produce vivid and copious studies that won Villari a very wide readership.

Villari did not confine himself, however, to academic work. Having been an ardent supporter of unification, he soon came to see that political unity did little to change social realities, especially in the

neglected south. In the *Lettere meridionali*, originally letters published in a Milanese newspaper, he did a great deal to awaken the conscience of a unified Italy to the social problems of the south. These concerns also lead him into public life as an educational expert, parliamentarian, and minister of education. Intellectually, he moved from idealism toward a positivist and empiricist emphasis on concrete study of social facts.

All these matters receive some attention in this brief study of Villari's writings. In a series of readings, Maria Luisa Cicalesse identifies the men that influenced Villari and the ideas that he made his own. But, for reasons I find hard to understand, Cicalesse does her best to avoid biography, leaving us with the faintest image of the man and a series of summaries, hardly more concrete, of his works. Birth and marriage, for instance, are mentioned in footnotes, though Villari's letters to his English wife (and translator) are a major source of his views. His public career is lightly touched on, and the letters published as an appendix to this volume are left largely without context.

It is never entirely fair to criticize an author for his or her omissions; nonetheless, it must be said that the narrow method of intellectual history adopted here seems particularly unsuitable. Traditional history of ideas is most useful when brought to difficult texts, most rewarding when applied to extraordinary minds. Villari, as Cicalesse is quick to say, was not a man of great profundity or originality. His interest, rather, lies in the concreteness of his ideas and concerns. Thus, summaries of his works that inevitably linger over his most general pronouncements convey only a weak sense of the man. Villari's own insistence as a biographer on connecting "the life and times" would have been a more appropriate model. The present work claims to be "Notes for a Profile"; it is only natural to hope for more.

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JOSEF POLIŠENSKÝ. *Aristocrats and the Crowd in the Revolutionary Year 1848: A Contribution to the History of Revolution and Counter-Revolution in Austria*. Translated by FREDERICK SNIDER. Albany: State University of New York Press. 1980. Pp. ix, 245. Cloth \$34.00, paper \$12.95.

The Czech historian Josef Polišenský is best known to readers of English through the translation of two of his previous works on the early seventeenth century. In *The Thirty Years' War* (1971) he sketched the broader conflicts in Bohemia and northern Europe and also examined the war's impact on a single Mo-

ravian village, Zlín. *War and Society in Europe, 1618–1648* (1978) discussed the importance of Czech archival repositories for the study of the Thirty Years' War and suggested new lines of interpretation. Polišínský has also written extensively on the diplomatic and social history of nineteenth-century Central Europe. *Revoluce a kontrarevoluce v Rakousku 1848* [Revolution and Counterrevolution in Austria in 1848] was originally published in Czechoslovakia in 1975. The title, somewhat embellished for the English version, derives from Friedrich Engels's articles that first appeared (under Marx's name) in the *New York Daily Tribune* in 1851–52.

Almost half of the book is devoted to an introductory historiographical essay (including a primer on Marxist-Leninist historical theory) and to tracing political and economic developments of the Austrian Vormärz. It is surprising that a historian of Polišínský's experience would repeat the view (discredited by modern scholarship) that Ukrainian, not Polish, peasants rose against the Polish *szlachta* in Galicia in 1846 (p. 18). Polišínský treats the revolutionary outbreaks in the streets of Prague and Vienna in June and October 1848 primarily as a backdrop to his main theme: the victorious march of the counterrevolution. In piecing together the various threads of ambition and intrigue that pervaded the court camarilla, Austrian cabinet, and Bohemian administration, Polišínský draws on the largely unstudied papers and correspondence of Habsburg aristocrats—notably Metternich, Ficquelmont, Windischgrätz, Schwarzenberg, and the Thun-Hohensteins—that were kept at their family estates in Bohemia. After World War II these materials were “acquired” by the Czechoslovak state archives. The timid bourgeois leaders, radical students, disgruntled artisans, and few industrial workers are shown to have been no match for Windischgrätz's loyal soldiers. Polišínský expressly sets out to debunk legends that have arisen concerning the Czech “national fathers” of 1848, whom he depicts as obsessed by the national question and fearful of social revolution. Karel Havlíček, for example, is derided for his defense of Windischgrätz's purge of radical Vienna. (Three months earlier Havlíček himself had been briefly imprisoned on Windischgrätz's orders.) The radical Czech student leaders, Josef Václav Frič and Vojta Náprstek, likewise receive little favor. “Heroes,” Polišínský concludes, “were not forthcoming from any of the ‘sides’” in 1848, and “not a few of the protagonists of the Czech national movement appear quite strange in retrospect. . . . The uniforms that have been invented for them will seem to have faded” (p. 210).

Frederick Snider's free translation of Polišínský's somewhat rambling text generally reads well. A comparison with the original Czech, however, re-

veals several lapses in the English version. For example, the authorship of Havlíček's celebrated article “The Slav and the Czech” is attributed to Jakub Malý (p. 130); Bohemian Governor Leo Thun, not Austrian Minister President Pillersdorf, attempted to delay the elections to the imperial Parliament (p. 166); *Pomoří* (the Littoral) is mistranslated as Pomerania (p. 200). Unfortunately, the book's appearance is marred by careless editing and an unacceptable number of typographical and sundry minor errors.

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DIETER LANGEWIESCHE. *Zur Freizeit des Arbeiters: Bildungsbestrebungen und Freizeitgestaltung österreichischer Arbeiter im Kaiserreich und in der Ersten Republik*. (Industrielle Welt, number 29.) Stuttgart: Klett-Cotta. 1979. Pp. 437.

The cultural emancipation of the proletariat constituted a central element of the socialist program in Central Europe. To promote this emancipation and the consciousness of class solidarity in general, the party sponsored numerous, wide-ranging educational activities and cultural organizations. The existence of this socialist subculture has long been known and frequently described. What distinguishes this new work is the attempt to penetrate beyond rhetoric and description to analyze the effect of the cultural program on the workers themselves.

Focusing on Vienna, the central socialist experiment in interwar, democratic Europe, Dieter Langewiesche evaluates the success of the efforts to create a “new man” primarily by examining the membership of workers' and people's libraries and the frequency and content of books withdrawn by the members for home reading. He deals more briefly with the content and attendance of lectures, courses, and films. The results, in part presented in 113 tables, are sobering and provoke a re-evaluation of the significance of socialist cultural activity. Belles-lettres constituted the overwhelming proportion of books withdrawn; the social sciences normally amounted to little more than 5 percent of the books read; classic Marxist works were seldom checked out. Moreover, there were no substantial differences in reading habits according to social group. Although the urging of socialist cultural bodies seemed to have effected a shift to novels embodying social criticism, they were unable to persuade large numbers of workers to read scientific or theoretical works. Similarly, pessimistic observations about lack of interest in demanding subject matter apply to the other educational and cultural activities of the movement. On the basis of these

findings Langewiesche argues bluntly that the bulk of the workers were ignorant of and indifferent to the theoretical discussions among the party elite that have hitherto attracted the interest of historians.

The reasons for the relative failure to produce an independent, socialist subculture were manifold. Chief among them for Langewiesche were the social conditions for work and family life; long hours, poor housing, limited finances, and inadequate primary education inhibited the average proletarian's participation in intellectual activity. In his nonworking time the worker needed and sought relaxation and recreation. Gradually, the socialist movement recognized this and sought to alter its program accordingly, but by then it faced a strong competitor: the capitalistic leisure industry.

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HARRIET PASS FREIDENREICH. *The Jews of Yugoslavia: A Quest for Community*. (Jewish Communal and Public Affairs.) Philadelphia: Jewish Publication Society of America. 1979. Pp. xiv, 323. \$14.95.

This work by Harriet Pass Freidenreich is a welcome addition to the scant available literature on East European Jewry. The reader should note, however, that it is not, as the title might suggest a general history of Yugoslav Jewry, but rather, primarily a study of three communities, Belgrade, Sarajevo, and Zagreb, ca. 1800–1941, and of national Jewish life between the wars.

In the first section of the work, which comprises a sketch of the earlier periods, the presentation, although basically sound, is marred by a few factual errors in the general Balkan background and by a discussion of the Yugoslav Sephardic community in which this community appears too detached from the other Sephardic communities of the Balkans.

The discussions of the clash of ideas over the nature of religious and national identity are exceptionally interesting since, in addition to the usual basic groupings of Orthodoxy, modernism, and assimilationism, the widely differing local Yugoslav nationalisms and a generational shift in attitudes among the Jews in the later nineteenth century added further complexities to the situation. The reviewer would have preferred greater elaboration on these subjects and less on the structural mechanics of the emerging Jewish organizations.

The second and third sections deal with social, cultural, and political life in the new united Yugoslavia during the period between the wars. Again, a fuller presentation of some of these topics, for example, Sephardi separatist ideology in Sarajevo and the rather unique role played in Yugoslavia by the

difference between Ashkenazim and Sephardim in the acceptance and rejection of Zionism, would have been of interest. (There should have been at least some specific bibliography on the Sephardi separatists.) In this section, also, one sees how differences among the three regions studied significantly affected Jewish involvement in local and national politics. An epilogue adequately covers the Holocaust and postwar period, although this reviewer feels strongly that some of the comments about Yugoslav Jewry today cannot serve any positive end and specifics will be avoided here so as not to exacerbate this further.

Freidenreich, for reasons unknown, decided to reproduce even the more familiar Hebrew terms used in the sources in their Serbo-Croatian forms. Surely at J.P.S. one would expect the editors to have corrected the non-Croatian transliterations (*haroshit* on p. 165), grammatical errors (*a bet hamidrash* on p. 33), and mistranslations ("*kfirim* [cubs]" p. 165); a *kfir* is a young lion, not a cub (*gur*), and the word was used in the Bible, in the youth movement to which Freidenreich referred, and as the name for an Israeli jet fighter to suggest youthful vigor, not crudeness.

The attention the author lavished on organizational frameworks indicates a belief that these are a significant indication of the strength of a community—a widely held view in America. The Yugoslav Jewish community, like the others in the Balkans, is clearly near its end. What will subsequent generations want to know about this community? The reviewer hopes the author will publish more studies from the voluminous material already collected on the spiritual quests of Yugoslav Jewry.

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MARK C. WHEELER. *Britain and the War for Yugoslavia, 1940–1943*. (East European Monographs, number 64.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1980. Pp. 351. \$18.50.

This excellent scholarly study will recommend itself only to the hardest general reader, but it is essential reading for all Yugoslav and resistance specialists. It concentrates on British relations with Mihailović, on why Britain recognized him in the first place, and on why the British continued to support him long after they knew about Mihailović's extreme Serbian nationalism, his commitment to civil war against Tito's Communists, and about Četnik collaboration with the Axis.

A central problem revolves around the reasons why the anti-Communist British leaders came to support Tito and abandon Mihailović. Mark C.

Wheeler's answers will not please everybody, but they are based on careful analysis of British official papers, including those from the War Office and Cabinet, besides the better-known Foreign Office material. The private papers of George Taylor (of the Special Operations Executive, the SOE) and material from the Yugoslav Military Archives have also been used. The author has discovered important new information and has given a lucid, detailed account of the complicated web of different, changing, and often conflicting views of British leaders engaged in policy making toward Yugoslavia. The first part of the study (pp. 16–61) about the coup d'état of March 27, 1941, is but an introduction to the real meat that follows. Wheeler takes the line of other recent research (that of David Stafford and Elizabeth Barker), giving perhaps too much credence to SOE claims. The author has little information about the crucial activities of the British Secret Intelligence Service (SIS), whose papers are never available, and whose members, unlike those of the SOE, are more closely bound to secrecy. This section is inconclusive.

The best part of the study begins with the start of resistance activities in Yugoslavia, which led to civil war between Tito's Communist Partisans and Mihailović's Četniks. It also led to prolonged disagreement between the British and Mihailović, whom the British wished to control for their war and postwar policies, regardless of the fact that he was singlemindedly pro-Serb and pro-Karadjordjević dynasty. Wheeler believes that the British abandoned Mihailović not because of Četnik collaboration with the Axis, which they knew about from the beginning, but because they came to the conclusion that he was not the man to organize either the resistance or a future Yugoslav state. The change in policy was reached reluctantly, hampered by the rear-guard actions of pro-Mihailovists, especially those in the SOE London office. The misreading or neglect of intelligence from Yugoslavia was often so blatant that it can only be understood on the grounds that until 1943 Yugoslavia had a low priority in the war effort. The author believes that British "arrogance" and an ingrained contempt for "Balkan" behavior affected British policy making until military criteria became paramount. Tito's successes in resistance and in the civil war against the Četniks as well as the developing war situation forced a change of policy. The British recognized Tito, abandoned Mihailović, and gradually and reluctantly gave up their aim of controlling the character and leadership of the postwar Yugoslav state.

Problems of Soviet Russia's involvement in this story are not elucidated and await future research.

PHYLLIS AUTY
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DOINO DOINOV. *Kresnensko-Razlozhkoto vŭstanie, 1878–1879: Prinos za negovnia obkhat i rezuitati, za vŭtrešniate i vŭnŭshnopoliticheskite uslovŭia, pri koito izubkhva, proticha i stikhva* [The Kresna-Razlog Rebellion; 1878–79: A Contribution to its Extent and Results, to the Internal and External Political Conditions in which it Broke Out, Ran its Course, and Subsided]. Summary in English. Sofia: Izdatelstvo na Bŭlgarskata Akademiia na Naukite. 1979. Pp. 333. 3.48 lv.

Doïno Doïnov's study is a continuation of his previous work, *Natsional-norevolutsionite borbi v Iugoza-padna Bŭlgariia prez 60-te i 70-te godini na XIX v.* (The National-Revolutionary Struggle in Southwestern Bulgaria during the Sixties and Seventies of the Nineteenth Century [1976]). In the preface and introduction to the current volume, Doïnov relates the present study to the earlier one and also examines the historiography, sources, and method used in his analysis of the uprising.

In examining the reasons, aims, course, successes, and failures of the uprising, the author concludes that in almost all respects this uprising represented a continuation of the struggle of the Bulgarians for national liberation that culminated in the formation of the Bulgarian principality. At the same time, it served to usher a new stage in that movement, particularly for the Macedonian and Thracian Bulgarians.

Doïnov shows that the immediate cause for the uprising was the dissatisfaction of the Bulgarians of Macedonia with the Treaty of Berlin, which abrogated the Treaty of San Stefano and thus placed Macedonia and Thrace again under Ottoman control. In tracing the unfolding of the uprising, the author discusses the strength and weaknesses of the leaders by focusing on the issues that divided the "internal" and "foreign" members of the leadership. He also shows the relationship between the military organs and institutions created by the rebellious people and the "Unity Committees" formed in the principality of Bulgaria to aid the rebels.

The external situation and the attitude of the great powers toward the uprising and the newly created Bulgarian principality are treated in a separate chapter. The author blames England and Austria for Russia's fear and failure to aid the revolutionaries. The revolutionaries both in Macedonia and in Bulgaria expected Russia to help them. Although most of the Russian administrators in Bulgaria sympathized with the Bulgarians and some actively supported the rebels, the tsar eventually came out against official aid.

The author believes that, despite its failure, the Kresna-Razlog uprising was important for the further development of the struggle of the Bulgarians for liberation from the Turks. Doïnov stresses the fact that the revolutionaries in Macedonia had a

Bulgarian national consciousness, regarded themselves as Bulgarians, and wanted to see Macedonia united with Bulgaria. This emphasis is in line with the general trend of recent Bulgarian writings on Macedonia.

The author is fully aware that this is not the definitive work on the uprising. There are still some issues that are not fully explained. The relationship between the leaders of the uprising and the "Unity Committees" based in Bulgaria and which of these groups had the last say on strategy and tactics still need to be elucidated. This reviewer would have liked to see a more detailed evaluation of the arguments presented by historians from Yugoslav Macedonia who regard the uprising as a purely Macedonian national uprising. This is a well-researched study based primarily on Bulgarian and Soviet archives and publications, and it is an important contribution to Bulgarian historiography. The book has a bibliography, summaries in four languages, and two indexes.

PHILIP SHASHKO
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I. P. SHASKOL'SKII. *Bor'ba Rusi protiv krestonosnoi agressii na beregakh Baltiki v XII-XIII vv.* [The Struggle of Rus Against Crusader Aggression on the Shores of the Baltic in the Twelfth and Thirteenth Centuries]. Leningrad: Nauka. 1978. Pp. 244. 1 r. 40 k.

Although the title and the author's qualifications might lead us to expect it, this is not a book about the Teutonic Knights, the "battle on the ice," and other tales of Germanic wickedness and Russian heroism. The latter is celebrated in full measure, but the bad guys in this book are Swedes. I. P. Shaskol'skii chooses to discuss what he calls the first two "waves" of Swedish aggression eastward—aggression affecting most directly Finns, Karelians, Ingrians, and the like, but also (and this is the author's emphasis) involving their "protectors," the Russians of Novgorod. Alexander Nevsky's victory in 1240 near the mouth of the Neva is a high point in the story, but it is Shaskol'skii's merit that he gives equal attention to all the obscure episodes in this century of conflict.

The primary material Shaskol'skii builds upon (some papal bulls, a couple of chronicles, some "lives") is very thin. After he finishes rejecting some parts, it is thinner yet. The fact is that we do not know very much; the size, the composition, the leadership, the ports of embarkation, the destinations, and even the dates of the various Swedish (and Russian) expeditions are in question. And intentions, in spite of papal exhortations and words like "crusade," are even more obscure. Shaskol'skii has the grace and the scholarly caution to admit

that on many points he can only offer hypotheses. But he helps himself (and gets a fascinating book out of it) by vigorously attacking the hypotheses of other historians (Swedish, Finnish, and even Soviet) and by presenting an impressive amount of "background" information. He hurls a formidable gauntlet and it will be interesting to see who dares to pick it up. Because the establishable facts are so few, virtually all the issues may be contested.

Shaskol'skii is probably most vulnerable because he is unabashedly on a crusade of his own. He is out to prove wrong all those "bourgeois" and "nationalistic" and otherwise Western-oriented historians who have suggested that the Finns, for example, were vulnerable to Swedish conquest because of age-old hostility toward the Russians. Shaskol'skii would have it that Novgorod Rus ruled mildly over the adjacent non-Russian tribes and helped them heroically in their struggles against Swedish aggression—an aggression that was only occasionally an attack upon Novgorod's heartland but always an assault upon its interests. Control of people and trade routes is the main issue, the feudal nobility of Sweden is the main instrument, but behind it is an expansionist Roman Catholic papacy that coordinated the aggression. Heroic resistance by the threatened peoples and their Russian supporters repeatedly foiled the aggressors who conquered Finland and part of Karelia only when Novgorod, weakened by internal problems and by the Mongol devastation of Russia, no longer gave massive aid to its non-Russian subject peoples. It is so much a cautionary tale that Shaskol'skii himself finds it necessary to disclaim current applications. Sweden, he says, is nice now, and the Baltic a sea of peace. His book, however, is bound to cause waves.

HEINZ E. ELLERSIECK
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A. L. SHAPIRO *et al.* *Agrarnaia istoriia severo-zapada Rossii XVI veka* [The Agrarian History of Northwestern Russia in the Sixteenth Century]. Leningrad: Nauka. 1978. Pp. 220. 2 r. 70 k.

This is the concluding volume of a collective trilogy devoted to the agrarian history of northwestern Russia in the fifteenth and sixteenth centuries. The first section completes the sixteenth-century regional studies by examining the northern Dvina, Kargopol' and Vaga uezds, the Lop areas, and the territory of Pskov; it has a final chapter on peasant crafts and trades. The picture that emerges is of a colonization largely completed before the sixteenth century; there were, however, adjustments, in part dictated by the requirements of agricultural technique, demographic factors, and such accidents as plague, invasion, and state exactions. Settlements

were predominantly of very small size, and tenements were mostly held by a single nuclear family. There were, however, considerable variations of wealth within this broad pattern, but most "peasants" (that is, dependents) who had large amounts of land were also engaged in fisheries, salt boiling, or iron making. The extractive industries were an important source of accumulation for the rich and enabled the poor to make ends meet. This dual function of gathering perhaps deserves greater attention. The lack of maps in this section (there is only one—of the Pskov area—and that lacks any indication of direction or scale) is regrettable.

The second section, little more than a third of the whole volume, draws some general conclusions on the economic developments surveyed in the three-volume work: population, settlements, trades and crafts, peasant holdings, obligations. A number of major problems arise here. Were losses of population and destruction of settlements as great as indicated in the late sixteenth and early seventeenth centuries? Were losses mitigated by population redistribution? Does the term "empty" (*pust*) in the registers of inquisition really mean just that? A shift to larger settlements and the increase in labor rents is associated with increased use of manure on compact fields around the settlement (p. 154). This seems, however, to demand further discussion of the motivation involved and calculation of the transport implications (sledges, carts, horses, hay, and so forth). There are interesting estimates of harvests per tenement in various regions which suggest that in many areas returns were inadequate; grain had either to be bought or replaced by other foodstuffs (p. 175). In these circumstances it was not so much difficult to take advantage of the high grain prices of the late sixteenth century (p. 180) as impossible to maintain a grain-based diet, yet the substitutes, especially the turnip, are inadequately dealt with. Money was not fully a medium of exchange in this society, and the peasant was often buying, not selling, grain.

The final chapter, titled "Progressive and Inhibiting Tendencies in Social and Economic Development: The Causes of the Late Sixteenth-Century Enserfment Measures," raises doubt on some issues. The dominance of three-field cultivation in the fifteenth century or earlier seems improbable (p. 185). In part this view distorts the interesting discussion of the reasons for the late emergence of capitalism in Russia. Peasant movement in Russia is seen as a form of class struggle restraining the lords' demands; the basic problem was the imbalance between productivity and sharply rising state demands resulting in intensified colonization and mobility, on the one hand, and enserfment as a countermeasure, on the other. This book is an essential work with much valuable material and a more

realistic approach than some others, but it also demonstrates that much remains obscure in Russian economic and social development.

R. E. F. SMITH

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A. I. KOPANEV. *Krest'ianstvo Russkogo Severa v XVI v.* [The Peasantry of the Russian North in the Sixteenth Century]. Leningrad: Nauka. 1978. Pp. 244. 2 r. 20 k.

This volume is another in the distinguished series of works by Leningrad members of the Institute of History of the Academy of Sciences on the peasantry in the northern and northwestern part of Muscovy. True to the tradition of the St. Petersburg school, the Leningraders usually have a better eye for the "facts" than do their Moscow counterparts, who tend to see events through a thick prism of dogma.

The central issue of A. I. Kopanev's book is whether the peasants of the North, the White Sea littoral Dvina Land, who had no seignorial landlords, were fundamentally in the same condition as their southern cousins, who did. A frequent Soviet argument is that all peasants were basically the same: the northern peasants paid taxes to the state, which used them for the benefit of the "ruling class," and differed little from seignorial peasants who paid rent directly to the members of the ruling class. Refuting that position, Kopanev shows that the nonseignorial peasants of the North owned the land and, as free persons managing their own individual, independent economies, were much better off than were peasants under the tutelage of a landlord.

Kopanev's second major thesis is that there was extensive differentiation among the Dvina Land peasantry, a "prebourgeois" stage, in which the wealthy peasants enjoyed political and economic control in the region, employed impoverished peasants as sharecroppers, and hired laborers to work for them. This thesis was earlier advanced by N. E. Nosov, the director of the Leningrad Institute of History, who is also editor of this volume. Much of Kopanev's work elaborates on and illustrates Nosov's famous 1969 work, *The Establishment of Estate-Representative Institutions in Russia: The Provincial Administrative Reform of Ivan the Terrible*.

Other parts of this valuable book show that the basic social unit was the nuclear family, refuting claims that the extended (*zadruga*) family existed in the North. Kopanev also shows that a three-field system of strip farming was in use (supporting his claim that there was a shortage of arable land), that barley was the primary grain, and that animal husbandry was of secondary importance. He demon-

strates the importance of manuring, but his claims that the peasants were trying to improve their crops by conscious selection are not convincing. (In the nineteenth century peasants still selected downward by selling the best grains, eating the intermediate, and sowing the worst.) Trapping, fishing, and salt boiling were also important in the North.

Kopanev discusses in detail some of the elite intermarrying families of the North (especially the Amosovs, Emetskiis, Molodois, and Shuigins), the likely descent of some from boyars and freeholding *svoezemtsy*, and their decline to the status of peasants because of equal inheritance practices. The degeneration of those clans was marked by a parallel decline in their ownership of slaves. Finally, the discussion of local self-administration is of interest, particularly where it touches upon the amount of attention that was devoted to the apportioning of the tax burden (sometimes dozens of meetings a year). It would have been useful had Kopanev discussed the ethnic composition of the region, the immigration of Russians, and their interactions with the native Finnish peoples.

RICHARD HELLIE
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JOHN T. ALEXANDER. *Bubonic Plague in Early Modern Russia: Public Health and Urban Disaster*. (Johns Hopkins University Studies in Historical and Political Sciences, 98th series, number 1.) Baltimore: Johns Hopkins University Press. 1980. Pp. xvii, 385. \$30.00.

After several sporadic outbreaks, an epidemic of bubonic plague struck the city of Moscow with full force in the summer of 1771, killing hundreds of people each day and causing untold numbers of others to flee for their lives. As the government tried to subdue the epidemic with various forms of quarantine, it succeeded only in intensifying the distress of the surviving population. At the peak of the crisis the common people rioted against the authorities, brutally murdered the archbishop of Moscow, and occupied the kremlin. Government forces soon suppressed the uprising, and with the onset of winter the epidemic subsided. Before it was over, the plague would claim an estimated 50,000 lives in Moscow, perhaps a third of total population, and 100,000 in the empire as a whole.

With exhaustive research and meticulous scholarship, John T. Alexander has succeeded in placing the central events of the Moscow epidemic into a historical context of impressive scope and detail. Among the subjects covered in this seemingly total reconstruction are the etiology of the plague and its progress from the Turkish front to Kiev, Moscow, and the Russian heartland; the physical condition

of Moscow, including climate, in 1770 and 1771; the status of medicine, epidemiology, and the medical profession in Russia; and the responses of the empress and other state officials to the crisis. Readers might desire more information about the common people's understanding of their affliction and their notions of how to deal with it, but there too the author has gone about as far as the known sources will allow.

Readers interested in the history of medicine as a science are especially well served by the author's sympathetic treatment of Russia's medical practitioners as they tried to apply their knowledge and training to the immediate crisis and then, when it was over, to understand and profit from the experience. Although all of them subscribed to some version of the miasmatico-contagionist theory of pestilence, they disagreed among themselves about its diagnosis, treatment, and prevention. The author concedes that their ministrations were ineffective in combating the epidemic, but their identification of filth and contamination as a source of disease-causing vapors may have helped in the long run to promote better sanitation and hygiene.

In assessing the consequences of the Moscow epidemic, Alexander finds that it disrupted the normal life of the city for several years, that it facilitated the planning and reconstruction of Moscow, and that it prompted Catherine to include provisions for the protection of public health in the provincial reform of 1775. As it appears in this work, however, the epidemic is less significant as an event with great consequences than as an event that tested and exposed the conditions in which it occurred.

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ROGER P. BARTLETT. *Human Capital: The Settlement of Foreigners in Russia, 1762-1804*. New York: Cambridge University Press. 1979. Pp. xvi, 358. \$45.00.

A fashionable, meaningless title ("Human Capital"), a period fashionable among present-day historians (the reign of Catherine II), and a fashionable interest in demography and immigration should not deter prospective readers from using a book that contains much useful material. It deals primarily with the agricultural immigrants lured to Russia by Catherine and her immediate successors. Urban immigration, treated extensively in studies of a Russian bourgeoisie, is touched upon only lightly; the important arrival of English and German merchants, for example, in Archangel is not mentioned. No comparison with immigration to America, which could enlighten the reader, is made. Roger P. Bartlett concentrates on the role of the state, on

government policies and laws. He points out that the policy "served the requirements of the state rather than the interests of the migrants" (p. 13)—a view that may need modification, for why should it not have served both sides, each in a different way? But, after all, the author's concern is with Russian policy and less with the immigrant, whose motivations, experiences, and fate do not constitute his topic. No reference is made to the intensive work of exploration, largely under Academy auspices, that helped to draw attention to the vast unsettled regions of southern Russia.

A great merit of the book is that it does not limit itself to Western immigrants—German, English, French (the latter constituting, as Bartlett shows, a small, interesting contingent), and even Americans, among whom an attempt at recruiting was once made. It also pays due attention to Turks, Armenians, Jews, Rumanians, Persians, Greeks, Serbs, and Kalmyks. Russian policy is the topic: its development, purpose, and organization, the recruitment of immigrants, and the conditions stipulated for them. Bartlett shows the transition from Peter I's aims of attracting entrepreneurial (artisan) and cultural forces to Catherine's goals of increasing population and promoting agriculture, wine production, silkworm raising, and sheep breeding. He also shows the change from inviting capable individuals to attracting compact foreign groups and, later, to reserving land for Ukrainians and Russians rather than increasing immigration from abroad. Military considerations, the author maintains, played only a lesser role.

When describing the settlements, the author contrasts theory, promises, and good will with practical results, successes with failures. Despite much that can be criticized in Russia's policy, despite bribery and unsystematic procedures, we gain a picture of thoughtful, reasonable, and magnanimous intentions. The settlers eventually flourished and Russia itself benefited—even if, as "pace setters and centers of innovation" (p. 214), they proved a disappointment, since Russians failed to imitate them.

Was the whole effort a chapter in mercantilism? The author does not seem to think so; he never mentions Colbert and refers only briefly to Prussian cameralism. And can we learn something from the Russian experience for today's situation with refugees and guest workers flooding many lands? It hardly seems so. The Russian situation, as so often, emerges as *sui generis*.

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F. G. SAFRONOV. *Russkie na severo-vostoke Azii v XVII-seredine XIX v.: Upravlenie, sluzhilye liudi, krest'iane, gorodskoe naselenie* [Russians in Northeastern Asia in

the Seventeenth to Mid-Nineteenth Centuries: Administration, Service People, Peasants, Town Population]. Moscow: Nauka. 1978. Pp. 257. 2 r. 10 k.

For some time a summary study of Russians in northeastern Asia—the territories of Yakutia, the shores of the Okhotsk Sea, Chukotka, and Kamchatka—has been needed. F. G. Safronov's work well fills this historiographical gap. Writing from a strong specialized-study base built by himself and other scholars of the problem, he has judiciously used central and regional archives to present a nearly complete picture of Russian activity in the area from its first joining to the tsarist state to the coming of capitalist society.

Safronov has divided his work into four chapters of varying lengths: the administrative history of the territorial divisions of the gigantic area; an examination of the composition and activities of the "service" people (*sluzhilye liudi*) who moved into the area; the history of agriculture and agriculturalists; and an examination of the town dwellers and their lives. The work is complemented by several town plans reproduced from the archives and a serviceable map of northeastern Asia. A full map of eastern Russia would have been helpful to the non-specialist reader.

Although the author does not specify it, the work presents a logical order that, in a way, is a thesis. In the second chapter Safronov admits the presence of "private" persons and their role in the opening of the Northeast, but, arguing against the emphasis placed on these private people by S. V. Bakhrushin and others, he says: "The chief role in this process always belonged to the service people who acted from the orders of and in the name of the power [of the tsar]" (p. 85). This, in part, comes from his rather wide use of *sluzhilye liudi*; it does not just include Cossacks. Also Safronov sees these service people as carriers (*provodniki*) of Russian power, as well as other things Russian. The agricultural conquest of the area offers, however, a slightly different picture.

During the period of introduction of settled agriculture to the Northeast, necessary because such a large area simply could not be provisioned from western Siberia and European Russia, there was no question that "a special place was occupied by governmental initiative" (p. 112). But not all of these settlers were forced; many were volunteers (*dobrovol'tsi*). In fact, he argues against V. I. Shunkov when he states: "The basic creators of Siberian arable land were voluntary settlers" (p. 109). Sometimes these settlers were "loose" (*guliashchii*) people who came to northeastern Asia for other reasons (primarily furs) but remained to settle. This did not, however, remove the government from the scene, and therein is Safronov's point. The tsarist govern-

ment was never far removed both from the original opening of northeastern Asia with the sending of the first two *voevody*, P. P. Golovin and M. B. Glebov, to Yakutsk, in 1638, and from the peasant settlement of the areas of arable land, which started with Golovin and Glebov and continued for the two centuries covered by the book.

Safronov's analysis of both groups of people is refreshingly free of class categories. He tends to work within classifications found in the documents. The same is done with the town populations in chapter 4. The town surveys in that chapter of Okhotsk, Petropavlovsk, and Yakutsk are descriptive and useful, although the latter two are expanded versions of his earlier articles.

The book certainly fills a historiographical need and is welcomed for that. It also continues the high standards of scholarship followed by Safronov in his work on northeastern Asia since 1953. In many ways it can be viewed as a summary of that work.

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MICHAEL JOSSELSON and DIANA JOSSELSON. *The Commander: A Life of Barclay de Tolly*. New York: Oxford University Press. 1980. Pp. ix, 275. \$37.50.

Tragically for Barclay de Tolly, during the past century and a half, the "obese, debilitated, and decayed" General Kutuzov has become transformed as the [G]reat Russian hero of the Napoleonic wars, from Tolstoi's fictionalized account to Stalin's imperialistic eulogies. After General Barclay de Tolly had ordered the evacuation of Smolensk in 1812, Alexander I was forced to choose Kutuzov—already sixty-seven years old and blind in one eye—as "Supreme Commander" of the Russian forces, because, as Michael Josselson explains, "[h]e was Russian to the core, and no other name could so effectively rally the anxious patriots, reassure the xenophobes, and turn the war into a national crusade" (p. 130). When Pushkin attempted to immortalize Barclay de Tolly in his ode, "The Commander," he was subjected to popular scorn and ridicule, despite the fact that in appreciation for his later effectiveness in the European campaign and the triumphal allied entry into Paris, Barclay de Tolly was awarded the titles of field marshal and hereditary prince (*kniaz*) by his sovereign.

Josselson has gone well beyond the inspiration of Pushkin in presenting the first, long-deserved, full-length biography of Barclay de Tolly. A persuasive portrait of the hero emerges against a thoroughly researched background of personal, historical, and military details. The devoted biographer (incidentally also of Baltic birth) has obviously spent many years gathering and piecing together a narra-

tive, persuasive in its argumentation and convincing in its documentation. Josselson found documents in French, German, and Finnish archives to supplement published accounts, including revealing personal letters of Barclay de Tolly from the Camphenhausen and Schroeder family papers in Germany.

Had he had occasion to work in Soviet archives, Josselson obviously would have found much more extensive unpublished documentation. The abridged nature of notes (regretfully hidden away at the end of the volume) and bibliography give little room to cite the mass of published sources or to comment on the historiographical controversies. There are lapses, too, in this respect; for example, when he mentions E. V. Tarle as an "independent Soviet historian" (quoting a French translation of Tarle), Josselson appears unaware of the modifications, forced revisions, and occasional contradictions in Tarle's published accounts, as revealed in articles by Leo Yaresh and Ann K. Erickson.

Josselson's justifiable preoccupations with the military strategy and tactics of his hero sometimes obscure broader historical problems. And many of Josselson's detailed battle descriptions would have been greatly enhanced by better maps, by diagrams, and by more sophisticated explanations of technical military considerations. Had Josselson lived through the editorial process himself (Diana Josselson is listed as collaborator for the final stages), perhaps such matters might have received more attention. Perhaps also there would have been fewer lapses in the equivalents provided for geographical names. (For example, Courland is part of Latvia, not Lithuania, and the use of the term "Livonia" is confused—and often used mistakenly—in nineteenth-century references.)

Such shortcomings aside, future historians will have a firm basis for the resurrection of Barclay de Tolly with Josselson's well-documented verdict. Along with the continuing inspiration of Pushkin, they can even take solace in the judgment of Marx and Engels, whom Josselson quotes (p. viii), to the effect that "Barclay de Tolly . . . was, beyond question, the best of Alexander's generals."

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IU. I. KIR'IANOV. *Zhiznennyi uroven' rabochikh Rossii, konets XIX-nachalo XX v.* [The Standard of Living of Russian Workers in the Late Nineteenth and Early Twentieth Centuries]. Moscow: Nauka. 1979. Pp. 285. 2 r. 20 k.

In the 1960s a group of scholars under the leadership of the late L. M. Ivanov did much to revitalize the study of labor history in the USSR. Iu. I.

Kir'ianov was one of its most prominent members, contributing a number of stimulating articles, a distinguished monograph, and a book-length bibliography on the condition and outlook of the pre-revolutionary working class. In the present work he has set himself the difficult task of determining how the standard of living of the Russian proletariat changed between 1880 and 1914.

Students of other nations' labor history will recognize many of the difficulties. Data are abundant but often ambiguous, incomplete, and unreliable. Evidence for different years and places was rarely compiled in a uniform, systematic fashion, and as a result long-term trends are hard to discern. Kir'ianov manages, however, to introduce a measure of order into this apparent chaos, judiciously weighing one source against another and offering cautious, tentative conclusions.

He limits his attention to four aspects of working-class life: hours, wages, food, and housing. In each case he reviews a wide range of sources (surprisingly few of them archival) and tabulates results from all parts of the Russian empire. He finds some evidence of material gains—the average workday, for example, was reduced by roughly two hours over the years of his study—but suggests that these were inadequate to the workers' needs, much less to their rising expectations. Improvements, moreover, were often illusory: workers moved from overcrowded, unsanitary factory barracks into rental accommodations that were even more squalid and unsatisfactory.

Where improvements did occur, Kir'ianov attributes them to the workers' rising militancy and class consciousness, but he may be overstating the case. For example, if wages rose between 1904 and 1909 (pp. 114–15), the explanation may not lie solely in the revolutionary upsurge of 1905–07.

In general, the author has kept to a narrow definition of his topic. Many related issues (technological change, composition of the labor force) are given short shrift, and the broader implications of some findings are understated or neglected (for example, the suggestion that wage differentials between the highest and lowest paid workers were diminishing over time). The book is a thorough compilation of evidence but not a novel synthesis. Specialists will value it for bringing together and summarizing a wealth of information that was previously incomplete or inaccessible. They will miss the lively argumentation that characterized the author's previous work.

R. E. JOHNSON
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PAUL DUKES. *October and the World: Perspectives on the Russian Revolution*. New York: St. Martin's Press. 1979. Pp. viii, 224. \$17.50.

ROY A. MEDVEDEV. *The October Revolution*. Translated by GEORGE SAUNDERS. Foreword by HARRISON E. SALISBURY. New York: Columbia University Press. 1979. Pp. xix, 240. \$14.95.

The Russian Revolution is usually treated as a local affair inspired by conditions peculiar to Russia and resulting in a new kind of tsarist despotism with a silver lining. This contrasts sharply with the treatment accorded to the French or American Revolutions, which are seen as world revolutions because they were liberal and, therefore, presumably more universal and legitimate. The two interpretive essays by Paul Dukes and Roy A. Medvedev transcend these clichés to view the Bolshevik revolution of October 1917 as an event in European or Atlantic culture, perhaps even as an event in global history. Medvedev, a Russian Social Democratic dissident, sees the October Revolution as a triumph for the European social democratic tradition, subsequently perverted by the policies adopted by Lenin after Brest-Litovsk in March 1919. Dukes surveys a wider span of time and tries to place Russia within the current of all the great revolutions of modern times, making its own contribution as the link in a chain of revolutionary changes going back four centuries.

There is much to be said for Dukes's notion that the Russian Revolution was as much a stage in the evolution of European or world society as an event in Russian history. He takes the reader through the crisis of the seventeenth century, the democratic revolutions of the eighteenth, the Industrial Revolution, and the twentieth-century revolutions in backward countries, demonstrating Russian involvement throughout (although he admits that Russia was more a recipient than a donor until 1917). The book is well written, sprinkled with apt quotations from recent scholarship, and thoroughly enjoyable.

Although Dukes's approach presents a cogently argued and refreshing alternative to most Western interpretations of the Bolshevik revolution, there are some aspects of it that are open to question. To say, as he does at the end of his book, that "In our estimation European anarcho-syndicalism had just as much to do with the Revolution as had the 'peasant wars' of the early modern period" is to go far beyond the evidence he presents. Dukes makes his point that the Russian Revolution derived from and influenced world history. But in doing so he has sacrificed depth for range. What is lacking is a characterization of the nature of the October Revolution, taking into account the most recent research on its social origins. Lacking that, it is presumptuous to apply labels.

The focal point of Medvedev's essay is narrow—the Russian Revolution in 1917–18—but his essay has some of the same strengths and weaknesses as the Dukes book. Although it is surprising for a resi-

dent Soviet scholar to criticize freely both American and Soviet scholarship, the range of sources cited by Medvedev is rather narrow, and the books are used to adorn his argument rather than to provide a solid floor of scholarship. Like Dukes, Medvedev presents an unorthodox thesis. Like Dukes, he fails to present enough evidence to prove his case. Like Dukes, Medvedev writes well and is interesting, even if the results are somewhat flawed by the impressionistic character of his approach.

Medvedev argues that the October Revolution was a potential victory for Marxist socialism despite the immaturity of Russian economic development. He is not arguing, therefore, that Lenin's "voyage into the unknown" was predestined to fail because of Russia's social and economic backwardness but rather that Lenin erred in creating a one-party monopoly of political power and in trying to move too quickly toward utopian socialism. In particular, Medvedev believes that efforts to institute too much state control in the economy too soon had profound repercussions, not only in the economy but also in determining the whole political evolution of the new Soviet state. He places a great deal of emphasis on the importance of trade, arguing that an earlier effort to encourage free trade in the country and in the cities might have forestalled the economic crisis that had such profound political consequences. Medvedev condemns especially the use of force in the countryside against the peasants and the formation of poor peasant committees, which he says "were not at all the vehicle of socialist ideas, nor could they be."

Medvedev presents an interesting thesis buttressed with Lenin's own self-criticisms, but one is inclined to ask whether this is not the wisdom of hindsight. Was the reintroduction of free trade even possible on the eve of the Civil War, since it would have created a dangerous area of freedom in a society preparing for siege? It is relatively easy to second-guess the Bolshevik leaders, but such exercises tend to presume that they had a greater degree of free choice than they actually did have. The pressure of events, their own lack of experience, the psychological orientation provided by their revolutionary careers—all pushed the Bolsheviks in the direction they pursued.

Both books are welcome additions to the historiography of the Bolshevik revolution. They have a place beside Crane Brinton's *Anatomy of Revolution* and Walter Laqueur's *The Fate of the Revolution*. Medvedev and Dukes jog the mind into a new perception of this event in modern history and they both present their arguments in a style and vocabulary accessible to the nonspecialist professional or literate layman. We could use more books in this genre.

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ALEXANDER J. MOTYL. *The Turn to the Right: The Ideological Origins and Development of Ukrainian Nationalism, 1919–1929*. (East European Monograph Series, number 65.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1980. Pp. 212. \$15.00.

An intensive examination of the 1920s as a critical stage in the evolution of Ukrainian nationalist movements has been long overdue. Alexander J. Motyl's notable accomplishment relates the disparate trends in Ukrainian politics and culture to the activity of Ukrainian political organizations in emigration and in Poland after the Treaty of Riga.

Motyl understandably focuses on ideological formulation and political organization at a relatively high level. The author clearly perceives the fundamental ambiguity of the relationship between the intellectuals who constituted the real force of the nationalist movements and the overwhelming peasant majority that constituted their nominal subject.

Motyl is mainly concerned with the increasing radicalization of Ukrainian nationalism that in 1929 produced the integral nationalist Organization of Ukrainian Nationalists (OUN). He perceptively relates this radicalization to the intense concentration on nationalist themes and goals that had characterized the Hetmanite "conservatives," the most influential émigré organization during the years immediately following the failure to establish a Ukrainian state. Motyl notes that the two aspects of Ukrainian nationalism differed primarily in their "vocabularies." Generally, however, Motyl does not relate Ukrainian nationalist themes to symbolic theory, which is perhaps the most innovative trend in current analysis of nationalism. Instead, Motyl places his subject in a framework derived from the historians of ideas who analyzed nationalism during the interwar period.

On many aspects of his subject Motyl is searchingly candid. He recognizes, for example, the tragic interrelation between Jewish and Ukrainian history. He contends that the intensification of animosity between the two ethnic groups constituted a vicious spiral for which prominent personalities on both sides were at fault. This interpretation affords a plausible starting point for an irenic approach to the vexing question of a specifically Ukrainian brand of antisemitism. Motyl is equally candid about the internecine conflict in the Ukrainian movement itself. He identifies the trends and ideas that prepared the ground for fratricide in the 1940s, but he fails to relate this trend to estrangement of nationalists and Ukrainian Catholic Church figures.

In his concern to demonstrate the domestic sources of Ukrainian nationalist movements and ideas, the author unduly minimizes external influences. Motyl rightly indicates the distinct ideological differences between Ukrainian integral national-

ism and fascism. He does not, however, make adequate allowance for the European *Zeitgeist* that contributed to the excesses of both. The analytic reader will, therefore, have to contribute much of the perspective for Motyl's findings. Nevertheless, his probing monograph will make it impossible for future synthetic treatments of nationalism to overlook the unique Ukrainian experience.

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CONSTANTINE KRYPTON. *Istoriia sovetskogo obrazovaniia i ego izucheniia v SShA* [Soviet Education: Its History and Its Perception in the USA]. Lakewood, N.J.: Kordume Press. 1978. Pp. 235. \$12.00.

Since the leaders of the 1917 revolution announced the production of the new man and began reorganizing the old school in order to produce him, American observers have been pleased and appalled by what they saw or failed to see in Soviet schools. During the 1920s and 1930s Columbia University and Teachers College were headquarters for the most influential American writers on Soviet education. Examining the writings of the Columbia school and related contemporaries, Constantine Krypton finds that the Hudson River experts consistently misread the fundamental trend of Muscovite education, just as Lunacharskii and Khrushchev misjudged it in their times. Krypton's analysis of the first forty years of Bolshevik educational policy and its running American commentary will enrage some, delight others, and provide anyone who reads Russian with a piquant review of a half-forgotten American literature on Soviet education, the best of it written when Dewey was still fresh and the Bolsheviks still young.

Part 1 of Krypton's essay is a concise and generally fair review of Soviet academic policy to 1964. It lays out the period of experimentation in the 1920s that appeared so hopeful to American progressives, the rigid reorganization of the 1930s that left them uncertain, and closes with the Khrushchev reform, which the author, in agreement with Soviet and Western critics, dismisses as academically unsound.

Parts 2 and 3 are a spirited polemic against Dewey, Counts, and to a lesser extent Kandel. Why did these intelligent men, one of whom (Counts) read Russian fluently and had direct access to the documents, miss the point of Soviet education as it struggled to encompass and serve Soviet reality? Krypton finds the answer to his question in the mind-set of the American educationists shaped by the history of American education: its contempt for European academic standards, its enthusiasm for the new, the facile, and the popular in the class-

room. Lacking a serious, classical-*real* tradition at the secondary level, American educationists misprized European bedrock underlying scholastic organization since the Crimean War: the political imperative that education is structured learning of intellectual skills that enable one to continue to master the scientific disciplines indispensable to world power. Everything else is just fooling around.

When Stalin organized the five-year plans tying Soviet schools to heavy production, he reconstituted the European-Russian academic school. Despite tampering at the hands of lesser politicians, it has remained the core of Soviet education and Soviet power. Progressive observers have difficulty interpreting a dictator with academic standards for the mass secondary level higher than their own. Krypton's assessment of the Americans is a shotgun blast, some pieces on target, some wide of the mark. His perception of the Soviet core is a marksman's bullet close to the heart.

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NEAR EAST

PATRICIA CRONE. *Slaves on Horses: The Evolution of the Islamic Polity*. New York: Cambridge University Press. 1980. Pp. ix, 302. \$39.50.

This comparatively short book consists of an essay, rich in ideas and brilliant in formulation (pp. 18-91), 711 learned notes, comprising approximately the same space (pp. 201-71), and a most variegated bibliography (pp. 272-87). Between the essay and the notes are squeezed six appendixes and a prosopographical analysis of tribal and military leaders, as well as of *mawālī*, or clients, of the Umayyad and early Abbasid periods (pp. 93-200). The often mentioned Rawḥ b. Zabda' should be spelled Zinba'. This section, partly taken over from the author's dissertation, should have been reserved for a more comprehensive publication. The "Historiographical Introduction" (pp. 3-17) is not introductory at all. Since the book makes the most minute use of Arabic historiography beginning with the later years of the caliph Uthman (ca. 650), there was no need to demonstrate the worthlessness of those sources for the preceding period of Islam. Moreover, the reasoning in this section is open to criticism.

I am unhappy also with the title of the book. Only the last of the ten sections of the essay deals with the "Emergence of the Slave Soldiers." I understand, of course, Patricia Crone's intention. The Muslim Arab "conquest society" destroyed the conquered population, first by outright enslavement and then by merciless taxation, forced them to give up their identity, and admitted them to the "con-

quest society" as "inferior and despised members, but as Arab Muslims." In the process, most of the genuine Arabs, becoming sedentary, joined the *misera contribuens plebs*, as an unarmed subject population, with the final result that from the middle of the ninth to the beginning of the nineteenth century the Muslim Arabs were under the domination of different types of slave soldiers.

But was this "Islamic Polity"? As the author herself says, "Truth and Power" were with the Arabs. They formed a religious and racial aristocracy. How this double asset was lost to the Muslim state is described by Crone with great finesse. According to her, this development was in the nature of the Arab conquest. Was it also in the nature of Islamic religion?

This study is to be lauded for putting Islamic developments squarely within the wider frame of world history, for instance, constantly adducing the successive conquests of China by the desert peoples of Central Asia. The ancient Near East would have provided other fruitful aspects of comparison.

The author is inclined to coin sharp formulations and delights in antithetical statements. Some pages read like an oration of Cicero. But life is complicated, and some readers, including this one, would have liked to have certain statements qualified by some more detailed considerations. This gifted young historian will have plenty of opportunities to provide us with some more extended products of her felicitous pen.

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MARILYN ROBINSON WALDMAN. *Toward a Theory of Historical Narrative: A Case Study in Perso-Islamicate Historiography*. Columbus: Ohio State University Press. 1980. Pp. viii, 214. \$15.00.

The major part of this book is a detailed analysis of the structure and content of a historical work of the Ghaznavid period (962–1186) written in Persian by Abu'l Fazl al-Bayhaqi (ca. 996–1077). The author of the work, commonly known as *Ta'rikh-i Bayhaqi*, was a secretary of the chancery under six Ghaznavid rulers. Hardly five volumes out of his thirty-volume history have survived; they cover mainly the reign of Amir Mas'ud (1030–1041) in some seven hundred pages.

In this first book-length study of Bayhaqi's history, Marilyn Robinson Waldman discusses the author's sources, method, and the structural features of his work, particularly his frequent interruptions of the central narrative with interpolations and flashbacks to previous periods of history. She analyzes his dominant themes of morality, human mo-

tivation, and royal power and shows that he lacked objectivity and often told stories to teach a lesson—sacrificing some accuracy. She also discusses his emotional language and portrayal, his journalistic and suspenseful style, his graphic imagery, and she assesses his place in the field of Islamic historiography.

Waldman actually uses her "multifaceted analysis" of Bayhaqi's text as a case study to criticize the way today's historians view or use historical narratives from the past as mines of factual information but do not allow them to provide abundant information about images of the past and about the history of language, ideas, and culture. She also tries to show how theories of narrative developed by literary critics, as in Mary Louise Pratt's "Speech Act Theory," could be useful in the study of historical narratives. Waldman successfully demonstrates that Bayhaqi not only reported the events, but also displayed them, responded to them, evaluated them, and invited his readers to join him in his response and evaluation, as in Pratt's description of "display texts." Waldman provides twelve translated selections from Bayhaqi's history in the appendix to illustrate the features and displays of his narrative.

Waldman has certainly contributed to the methodology of the study of historical narrative in her thorough and erudite analysis. It is unfortunate that she chooses to use the bizarre and grammatically incorrect term "Islamicate" to describe the products of civilization associated with Islam instead of the universally used term "Islamic." Some necessary corrections are: the Zaydis of Yemen (p. 48, n. 4) ruled 898–1962, not 893–1300; the translation of Nāsir al-Dīn (p. 45) is "upholder of the religion," not "conqueror of the religion"; Husayn-i Ali (pp. 171, 174) was the grandson, not the cousin and son-in-law, of the Prophet Muhammad; Bayhaqi's birth ca. 966 (p. 4) contradicts the date ca. 996 (p. 39).

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CARTER V. FINDLEY. *Bureaucratic Reform in the Ottoman Empire: The Sublime Porte, 1789–1922*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1980. Pp. xxxiii, 455. \$25.00.

The Ottoman state owed its long existence to, among other things, a bureaucratic apparatus that functioned both as a social class and as an administrative body. Yet, despite the role played by this bureaucracy in the history of the some thirty-odd states that were the successors to the Ottoman state, few scholars showed the temerity or had the required skills to study in depth this difficult topic. Of course, there are the studies of the late I. H. Uzun-

çargili, Albert H. Lybyer, H. A. R. Gibb and Harold Bowen, and Joseph Mautz, just to cite a few important ones, but these dealt only with some aspects or branches of the Ottoman bureaucracy during specific periods. Carter V. Findley is the first scholar to have studied fully the evolution of the scribal service that, alongside the rise of the Sublime Porte, changed from an obscure office with barely seventy-three people in the sixteenth century into a reformist bureaucratic elite and, ultimately, into an army of civil bureaucrats numbering fifty thousand to one hundred thousand people from 1876 to 1909. The book, written in a clear and even elegant style is divided into eight chapters: the first two attempt to place the Ottoman bureaucracy in a broad institutional-historical and cultural framework, while the remaining chapters (followed by a conclusion) analyze in a chronological manner the metamorphosis of the humble scribes into a powerful body of modern bureaucrats. The author touches upon other branches of the Ottoman bureaucracy only to the extent they relate to his topic; the military bureaucracy is not studied at all.

Findley has studied what was essentially the transformation of the imperial Ottoman patrimonial bureaucracy into a rational legal system, beginning chiefly with the reign of Selim III. He claims that the scribal service created in the sixteenth century under the *Reisul Küttap* (chief secretary) to perform chancery services for the imperial *Divan* (council) was the chief force in this process of bureaucratic transformation whereby "elements of the ruling class that had been only marginally distinguishable or miniscule in size now emerged with distinct form and unprecedented power" (p. 48). Literary traditions (*adab*) that blended with the influence of the dervish orders and, especially, of the guilds determined the organizational and behavioral patterns of the scribes.

During the nineteenth century, under the stewardship of the *Reisul Küttap*, who conducted foreign affairs and often rose to premiership, the scribal class became both the foundation of a new class of intelligentsia and the instrument for introducing modernist reforms.

This is an excellent study. Findley investigated his topic with painstaking care, meticulously scrutinizing a great variety of difficult original documents from the Turkish archives as well as published sources. He shows not only erudition and understanding of the Ottoman political culture and behavior but also insight into the social origins and personalities of the Ottoman bureaucrats. Consequently, I find it difficult to understand why Findley searched for conceptual and theoretical guidance not in his own original research and findings, but in the works of other scholars whose knowledge of Ottoman bureaucracy was partial or

insufficient, or who strived pretentiously to cast this bureaucracy into preconceived models. For instance, he makes continuous references to Max Weber's work although Weber had a very scant and, for the most part, biased knowledge of Islam (to which the Ottoman bureaucracy owed so much) and of the special patrimonial features in the class of the sultan's servants. Thus Findley refers to the Ottoman bureaucrats as the "slaves" of the sultan because the sultan appeared outwardly to have an absolute authority over the life and property of his officials. Actually, the Ottomans made a clear distinction between *köle* over whom the owner had proprietary rights and the *kuls* or men who had a permanent quasi-contractual and personal relationship with the sultan.

Findley fails to assess fully the importance of the economic and social forces that effected the transformation of the Ottoman bureaucracy. For example, he attributes the ascendancy of the scribal order and the eventual birth of a civil service to the deterioration of the old imperial system, the need of the bureaucratic elites to protect themselves against the sultan, and so forth. He does mention the "new kinds of problems facing the empire" (p. 61) and the fact that "government business was growing in volume and seriousness everyday" (p. 116), but he does not analyze in depth the impact of these forces on the Ottoman bureaucracy. The fact is that the transformation of the Ottoman imperial bureaucracy into a civil service beginning at the end of the eighteenth century coincided with the final disintegration of the old social estates and their agrarian basis, with the emergence of the new social groups, and with the eventual incorporation of the Ottoman state into the capitalist economy of industrializing Europe as a raw material-producing, dependent system. The bureaucracy played a key role in achieving this state of dependency, which was often cloaked in the garb of reformism. The high Ottoman bureaucrats became the beneficiaries of—if not the outright partners in—the new capitalist relations.

Obviously, old traditions such as the "patrimonial tradition of control over the distribution of economic resources" (p. 162), as Findley put it, helped the bureaucracy play a vital role in the emergence, as well as the maintenance, of the Ottoman capitalist system. Nonetheless, Findley would have enhanced further the value of his excellent book if he had freed himself from concepts unsuitable to the study of the Ottoman bureaucracy and paid closer attention to the prevailing socioeconomic and political forces that changed not only the bureaucracy but also the Ottoman state as a whole. One may object further to some of Findley's views on reforms, constitutionalism, and the Hamidian and Young Turk periods. To do so, how-

ever, might downgrade the value of the book. Basically, this is an outstanding contribution to Ottoman studies both as a source of information and as a model of scholarship. It deserves full praise and use by all students of the Middle East and Balkan history and society.

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G. A. NERSESOV. *Diplomaticheskaiia istoriia egipetskogo krizisa, 1881–1882 gg., v svete russkikh arkhivnykh materialov* [A Diplomatic History of the Egyptian Crisis, 1881–82, in Light of Russian Archival Materials]. Summary in English. Moscow: Nauka. 1979. Pp. 318. 3 r.

The revolution led by Arabi Pasha in 1881–82 pitted traditional Islamic culture against the liberal reforms advanced by Westernized Egyptians and foreigners, against foreign influence generally, and against British interests attached to the economic and strategic importance of the region and the Suez Canal. It is an unforgettable event in Egyptian history that also commands attention in studies of European involvement in Africa, the Eastern Question, the nature of Victorian imperialism, and Gladstone's controversial Liberal foreign policy. G. A. Nersesov is thoroughly familiar with Western interpretations and knowledgeably engages the essential issues as an advocate of both the Marxist-Leninist theory of imperialism and of national revolution based on traditional culture. His approach is typical of current Soviet studies of Asian and African history, and it is not as contradictory as it might appear.

Nersesov has not written a comprehensive history of the crisis but rather a study of the Russian role "in the light of Russian archival materials." These materials are a source of new information about Russian relations with most of the European powers and Turkey as well as Egypt. The Russian government opposed British or joint British-French intervention to suppress the revolution. Nersesov is at his best when he adheres closely to his sources in discussing the Russian policy, from the negotiations that led to the agreement with Germany and Austria-Hungary in October 1881 to the two stages of the Constantinople conference. He makes a strong case that Russia desired a settlement by the powers in concert. He attributes the Russian failure to the divisive ambitions of the Western powers, Germany included, and to the rise of the "new imperialism" and competition leading to world war.

Although Nersesov exhibits a pragmatic, ideological openness that is becoming more frequent in Soviet international studies, his analysis serves to

bring out the familiar lessons to the point of more than implying that his subject has contemporary relevance. He goes much too far in this, lacing his text with unsubstantiated inferences—for example, that at some deeper popular level Russia sympathized with the Egyptian cause. Tsarist Russia had no affinity for the Islamic world.

Nersesov writes that the Egyptian crisis was one of those episodes that can be understood only in a larger international context and that it was directly related to the Eastern Question. Yet his greatest difficulties, and the hapless inferences he wantonly interjects, might have been avoided had he dealt with Russian policy in the larger context. He gives only a very hazy impression of Russia's real or perceived interests in the Near East and North Africa and of the consequent Russian influence on British policy. When Gladstone reluctantly decided to intervene, he was following a policy and a concept of security that had been laid down by another complex, controversial minister, Palmerston, whose preoccupation with Russia bordered on phobia. Nersesov has skirted widely around one of the most formidable issues involving Russian policy in this crucial period—that in rivaling the Western powers and opposing British imperialism it was no less contentious for being conservative and ineffective.

HAROLD INGLE
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RASHID ISMAIL KHALIDI. *British Policy towards Syria and Palestine, 1906–1914: A Study of the Antecedents of the Hussein-McMahon Correspondence, the Sykes-Picot Agreement, and the Balfour Declaration*. (St. Antony's Middle East Monographs, number 11.) London: Ithaca Press, for the Middle East Centre, St. Antony's College, Oxford. 1980. Pp. xii, 412. £11.50.

This monograph should be read by specialists because it provides important documentation on the years immediately before the war, puts that information into historiographical perspective, and poses interesting problems for further research.

The book begins with the 'Aqaba incident of 1906, when British rule in Egypt was challenged by Ottoman authority. Although Anglo-Ottoman diplomacy eased the tension, the incident stirred British fears about the defense of Egypt and the loyalty of Egyptians. London and Cairo, re-examining the Syrian-Palestinian situation also in light of Baghdad and Hejaz railway construction, explored a variety of strategic and political options for the defense of Egypt. In documenting Anglo-French railway schemes for Syria in 1909–10, Rashid Ismail Khalidi demonstrates how London found the claims of France to the area as clear as Cairo found the Syrian situation confusing, which made London

wary of British involvement in Syria and Cairo eager to have a Palestinian buffer for Egypt. When the Young Turks imposed Turkification, the old notables and religious leaders in Syria looked back nostalgically on the days of 'Abd al-Hamid, while the young Arab soldiers and journalists became angry at Turkish administration and its acceptance of Zionist immigration. Some British saw Syria as fertile political ground until they observed French diplomatic pressure and Turkish political repression in action. Although Kitchener interceded on behalf of 'Aziz 'Ali, a Circassian born in Egypt, educated in Turkey, and secret nationalist leader of Arab soldiers, most British could not make sense of such complexities, which made an alliance with Hussein in the Hejaz seem a better Arab alternative for the British than Syria.

The foreword by Albert Hourani, Khalidi's dissertation supervisor, states: "We know by now something of what the British thought about the Arabs, and of what Arabs thought about the British and Turks, but what the Turks, and in particular the Turks of the Committee of Union and Progress, thought about the Arabs is still largely an unanswered question." Khalidi, arguing that Elie Kedourie's scrutiny of the war and its aftermath is insufficient for understanding British policy, shows how many war policies had already been anticipated by the British before 1914. For example, from Cairo Kitchener cast a big shadow over British policy in the Middle East before he became a powerful figure in London during the early years of the war. Khalidi's treatment of other, less prominent British officials is equally astute. As for the Arabs, he challenges the romanticism of George Antonius, who saw Arab "awakening" everywhere, as well as Ernest Dawn, whose realism may well have missed the extent to which rage had manifested itself in Syria over Turkification and Zionist immigration before 1914. Khalidi knows that much more research (and documentation) is necessary to bring the Arab picture into historical focus, but his criticism of the Arab leaders appears apt.

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DAN V. SEGRE. *A Crisis of Identity: Israel and Zionism*. New York: Oxford University Press. 1980. Pp. x, 182. \$14.95.

This work is another addition to the voluminous and exhaustive literature on Israel. Most of the works that have been written thus far, however, are general accounts dealing with broad topics involving historical, economic, political, and social issues. Dan V. Segre's book, *A Crisis of Identity: Israel*

and Zionism, is more of a specialized work. It concerns itself with a specific problem—the identity crisis in contemporary Israel. Segre cogently analyzes the inner struggles within Israeli society; he discusses the causes and offers possible solutions.

Zionism has generally been viewed by both its supporters and detractors as the political-national movement of Jews in Europe, which has been influenced by European historical and ideological experiences. The political structure of the Zionist movement resembled the European parliamentary system of multiple ideological parties representing all shades of political, social, and economic views. Subsequently, when the state of Israel was created in 1948, it based its political, social, and economic system on the Western model.

The author therefore maintains that the current domestic crisis in Israel, as expressed by its economic difficulties, crises of leadership, social divisiveness, and Israeli isolation in the community of nations, could be attributed to the country's break with Jewish past traditions, culture, and values, and its eagerness to emulate the West. Segre advocates a return by Israel to its ancient heritage. Israel must serve as a shining example to other nations, and its philosophical basis needs to be its religious and cultural heritage as expressed by the Torah (five books of Moses), Prophets, and Talmud (oral law).

Segre also suggests that the basic course of Israel's international problems has to do with its desire to be like any other nation. This has hurt Israel both domestically and internationally. At the end of the book the author offers his own solution. He advocates that Israel redefine its cultural and political values and return to its past heritage.

I find this book to be one of the most important works that has been written on Israel. It deals with the crux of the problem—self-identity and the crisis of political leadership. It should be pointed out that the author himself is a nonreligious Jew, educated and raised in the Western tradition. Yet he maintains that for Israel to survive as a unified nation with a high moral standard, it must return to its ancient heritage and not base its society on Western tradition.

From a structural point of view, the book is written on a high level. The author expects readers to be familiar with Jewish and general European history as well as to understand the background of modern Israel. The book, therefore, is too specialized and appeals to a selective group—the specialist on Israel and the Middle East. It would not appeal to the average reader. Overall, I recommend this book very highly as an indispensable work on contemporary Israel.

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AFRICA

PETER WOODWARD. *Condominium and Sudanese Nationalism*. Totowa, N.J.: Barnes and Noble. 1979. Pp. xiv, 221. \$22.50.

A salient feature of the world's history since 1945 has been the eclipse of European imperialism and the concurrent rise of scores of independent political entities throughout the Third World. This movement has been accompanied by worldwide historiographical scrutiny of the twin phenomena of de-colonization and the maturing of a variety of Afro-Asian nationalisms. Although there have been attempts to develop an eclectic, comprehensive explanation of the moves toward Afro-Asian independence, most published studies have tended to adopt one of five approaches: (1) focusing upon the rise of a particular nationalism and the resulting political and constitutional consequences; (2) economic treatments revolving around the implications of underdevelopment in the Third World; (3) works emphasizing the post-1945 international political environment and the way superpower rivalries have conditioned nation building in Afro-Asia; (4) social analyses focusing upon indigenous elites or classes; (5) treatments concentrating upon interaction between imperialists and local peoples inside a given area, especially those involving collaborative relationships.

Peter Woodward's account of the achievement of Sudanese independence clearly falls in the fifth category. Still, the author takes pains to note that events within Britain and the larger international environment influenced the evolution of collaborative relationships inside Sudan (such as those involved in establishing a local civil service). The book is chronologically arranged into eight chapters. The first seven of these deal with the evolution of the Anglo-Egyptian condominium—established in 1899 after the defeat of the fundamentalist Islamic Mahdist movement—into an independent Sudan in 1956. Here the interaction among Britons, the younger generation of Mahdists who created the first Sudanese political parties, and locals recruited into the new Sudan Political Service is detailed. So, too, is the abortive Egyptian attempt to influence the Sudan to accept a political union of the two peoples. The final chapter is short but insightful. It points to the continuing—if often obscured—validity of habits and relationships born in the 1899–1956 period.

The book is solidly based upon extensive research in major Sudanese and British archives and benefits, too, from the author's first-hand knowledge of the country. Woodward has not produced a comprehensive history but a carefully delineated dis-

cussion of the attainment and subsequent nurturing of Sudanese independence. It will contribute much to a broadly conceived, definitive history of modern Sudan when time allows such a work to appear.

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Knoxville

JAN HARM BOER. *Missionary Messengers of Liberation in a Colonial Context: A Case Study of the Sudan United Mission*. (Amsterdam Studies in Theology, number 1.) Amsterdam: Rodopi; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. 530. \$49.50.

Discussions of European missionary enterprise in Africa and elsewhere have been bedeviled by a paradox aggravated by the increasing attention that has been paid to the subject. Traditionally, as this book clearly demonstrates, Christian missionaries in foreign lands have perceived themselves as humanitarian forces, whose mission was to liberate the natives from ignorance, oppression, and woe through the Gospel. Yet outside of the missionary circle, the impression persists that Christian missionaries are far from being liberators; rather they are seen as tools of and participants in European imperialism. It is this latter perception of the missionary that prompts Jan Harm Boer to recommend a radical transformation of the attitude and behavior of missions and churches in this neo-colonial era. Boer argues that "missionaries must represent a community that offers clear and viable alternatives to competing ways of life in today's global village. The church must shed its conservative reputation of being in league with oppressors" (p. 484).

Written obviously from a missiological perspective, *Missionary Messengers of Liberation in a Colonial Context* focuses attention on the activities of the nondenominational Sudan United Mission in northern Nigeria between 1900 and 1960. While Boer does not pretend to blaze a new trail in this study, the reviewer, at least, found the book quite refreshing, particularly in terms of the insights it provides and the wide range of topics that are discussed. Of special interest is the attempt that Boer makes to relate the behavior of the missionaries to their sociological and ideological background. Using this as the backdrop for his analysis, the author bemoans "the terrible consequences" of S.U.M.'s endorsement of British colonialism in northern Nigeria. Directing attention to the chief *dramatis personae*—Kumm, Maxwell, Farrant, and others—Boer shows very convincingly how their ideological orientation led them to support British colonialism

blindly. Thus, despite claims by church and state to be progenitors of "civilization," both are inextricably tarnished and are viewed as colonizers and oppressors.

Indeed, regardless of the contributions of church and state in the social and economic development of the African people (if that can be agreed upon), the Africans seem to believe that, in the sphere of education and politics, the "ideas" that the "civilizers" have brought "were foreign to their own training and that of the ancestors" (pp. 292, 295). More importantly, Boer questions the claims of the S.U.M. missionaries as liberators. If indeed they were, why, he rightly asks, did they not enthusiastically support African nationalist movements? For "when the victims of poverty and oppression begin to assert themselves . . . by seeking to change these circumstances, the Church's representatives (often) withdraw" (p. 475), equating nationalism with evil and Communism (p. 390). This is an interesting point that scholars like Adrian Hastings (*A History of African Christianity* [1979]) and L. Newbigin (*The Relevance of Trinitarian Doctrine for Today's Mission* [1963]) have also criticized.

One disappointing aspect of the book, however, is its structure. Even a casual glance at the table of contents will reveal that it reads more like a book of reference than a monograph in the conventional sense. Worse still, many of the chapters are unusually long: chapter 4, for example, is 108 pages long. Chapters 6 and 7 are 62 and 56 pages respectively. But despite a few errors of fact (pp. 231, 234, 235) and stylistic infelicities here and there, this is an informative book, rich in factual detail and bibliography. Historians and missiologists will definitely find it rewarding to read. My only regret is that a book of this size can be reviewed in no more than 450 words!

FELIX K. EKECHI
Kent State University

W. G. CLARENCE-SMITH. *Slaves, Peasants and Capitalists in Southern Angola, 1840-1926*. (African Studies Series, number 27.) New York: Cambridge University Press. 1979. Pp. xii, 132. \$17.95.

In this reviewer's opinion, this scholarly new study of southwest Angolan history is an example of how a rigid theoretical framework can mar a body of superbly researched material. W. G. Clarence-Smith of York University revised his 1975 doctoral dissertation at London University, convinced that the dissertation's simpler approach required what he terms, "a systematic theoretical interpretation," a lucid euphemism for a Marxist class analysis. Undoubtedly this little book is the product of the most exhaustive historical research yet accomplished on this region of Angola. Although Clarence-Smith

does not deal with all of "southern Angola"—indeed, the title is misleading here—his portrait of the society and economy of southwest Angola is comprehensive and at times masterful. It is a pity that Clarence-Smith's application of his theory is so rigid that he neglects any reasonable consideration of important political and cultural factors which shaped that Angolan region during 1840-1926.

One example will have to suffice to illustrate how class analysis as applied in southern Angola terribly oversimplifies the problem of causation. On page 44 Clarence-Smith suggests that the reason why a number of Boers—South African whites who had trekked to Angola in the post-1880 era—refused to send their children to Portuguese schools was because of economic necessity. In this case no mention is made of the deep antagonism, both political and cultural, felt between Boer and Portuguese, an essential point mentioned in all the historical sources.

As an economic history of one period and part of one region of Angola, this is a pioneering contribution to African history. Despite Clarence-Smith's failure to understand that nationalist and political motives were at least as potent as economic ones in Portuguese colonialism, his mini-biographies of several key Portuguese traders are revealing and useful. No other historian has studied the sources for this region so thoroughly; nor did he limit his searches to only one archive. He has gleaned much from relevant French, British, Portuguese, and Angolan archives and has mastered the surprisingly extensive published literature. Especially useful for social history is Clarence-Smith's mastery of anthropological studies. Chapters 5 and 6, "Peasant Economy" and "Peasant Societies," are fascinating. The reader finishes the work with a thorough grasp of how southern Angolan tribes organized social and economic structures, various modes of production, trade, and commerce.

The study's most thorough discussion is of the period of 1840-1910 when slave trade and slavery were still important factors in the economy. There are some errors of fact in the study of the first republican period (1910-26), including the allegation that the 1911 elections were "free and serious," and a study of the full impact of the state on society and economy after 1926 is left to other students.

DOUGLAS L. WHEELER
University of New Hampshire

ASIA AND THE EAST

ARTHUR P. WOLF and CHIEH-SHAN HUANG. *Marriage and Adoption in China, 1845-1945*. Stanford: Stanford University Press. 1980. Pp. xx, 426. \$18.95.

Works on Chinese culture usually present a view of traditional Chinese marriage that includes the following features: people gave their daughters out at

marriage and took in their place wives for their sons, adultery was fairly strictly controlled, and divorce was very rare. In a scholarly tour de force, Arthur P. Wolf and Chieh-shan Huang demonstrate that, for one corner of the Chinese cultural world at least, these generalizations are all inaccurate. Using a combination of data from Japanese colonial-period household registers on Taiwan and field observations and interviews conducted since 1958, the authors show that, in the early years of this century marriages based upon a son-in-law moving in (uxorilocal marriage) or adopting an infant daughter-in-law to raise as an eventual bride for one's son (minor marriage) together were more common than the orthodox virilocal marriage of young adults (major marriage) in a nine-district area outside of Taipei city. The authors also find evidence of more adultery and divorce than the orthodox picture of Chinese marriage suggests. Their book presents a detailed analysis of the dynamics and social consequences of these variant marriage forms and a set of explanations for why families resorted to one form rather than another.

The authors devote much of their attention to the mysteries of minor marriage, which contrasts most dramatically with the major marriage form. The Japanese-period household registers they rely on for their primary evidence are a veritable gold mine for the study of family history. These registers make it possible to trace the changing composition of the 1,478 families that originally populated the nine-district area over the period 1905-45. Not only basic life events for each individual in these families are recorded but also such things as whether each was an opium addict, had been vaccinated, or had bound feet. An immense amount of work was involved in collecting and analyzing the data from these registers, but the result proves that it was worth the effort. Wolf and Huang's book reads like a detective novel in many places, as numerous clues to the mysteries of Chinese marriage customs are tested against the wealth of information in the household registers and accepted or rejected. The effect on the reviewer is somewhat intimidating, since there appear to be few explanations for the variant marriage customs that the authors have not already considered and tested.

In the end, however, one very large puzzle remains. The area Wolf and Huang studied appears to have had unusually high proportions of both minor and uxorilocal marriages. Even within Taiwan there appear to be areas where these variant marriage forms were much less common. The authors present data for the China mainland that, although sketchy, indicate substantial regional variations in the popularity of the different marriage forms there as well. But why should people in one Chinese community practically prohibit the variant marriage forms while in another they allow or even prefer

them? (The authors argue that minor marriage was actually preferred in their area over major marriage, although here they seem to stretch the evidence a bit.) Wolf and Huang are able to reject some possible reasons, such as the extent of poverty in an area, but they are only able to hint at alternatives. In the fall of 1980 Arthur Wolf and his wife, Margery, set off on field visits to selected locales in the People's Republic of China, and perhaps they will be able to fill in this major piece of the puzzle.

Wolf and Huang's study clearly ranks as a landmark piece of research on Chinese culture and family life, one whose conclusions no future scholar will be able to ignore. The meticulous care they use in forcing us to consider the unrecognized novelties of Chinese marriage customs presents a lofty standard for future scholarship. Their work also represents a major contribution to the study of family history outside of a Western context. Researchers on family life in the West have much to learn from this study, even as they envy the richness of the detailed data upon which it was based.

MARTIN KING WHYTE
University of Michigan

KENNETH G. LIEBERTHAL. *Revolution and Tradition in Tientsin, 1949-1952*. Stanford: Stanford University Press. 1980. Pp. viii, 231. \$18.50.

Once in a while, a book comes along that either breaks new ground or makes important contributions to scholarship. This book succeeds in doing both: it is the first serious case study of a major urban area in the early years of the Chinese Communist revolution, and it adds considerably to our knowledge through an in-depth examination of Chinese Communist politics in action, which may have wide implications for the future. At a time when many sinologists in the West are still recovering from the embarrassment of seeing their earlier vaunted works rendered inoperative by the current candor in China, this hard-headed study provides a timely and refreshing change.

As the title of the book implies, this is a study of how the revolutionary forces coped with the forces of tradition in a unique urban setting and the extent to which accommodations had to be made for the purpose of social transformation. One of Kenneth G. Lieberthal's most important findings is that, despite the sound and fury, convulsive campaigns are not as effective as calculated organizational efforts in doing the job.

Growing out of a 1972 Columbia dissertation, the book is tightly organized around several major topics in chronological order, dealing with strategies, the suppression of counterrevolution, economic reform, the campaign to aid Korea, the "Three Anti" and "Five Anti" campaigns, and the Thought Re-

form. Each chapter is carefully written, with amply documented facts and cogently reasoned analysis. For primary sources, the author relied on contemporary newspapers, official reports, and personal interviews.

Among other lessons to be drawn from the Tientsin experience are that the Chinese Communists are neither as strong as they seem nor always successful in combating traditional forces and that rationalization does not mean liberalization. The compatibility of methodical organizational control and political dictatorship is not a comfortable prospect for those who prefer freedom of choice. Indeed, Lieberthal believes the Tientsin case study suggests that "the splurge of rule-making that has occurred since the death of Mao Tse-tung and the overthrow of the radicals will enhance rather than constrain the successor leadership's ability to transform Chinese society" (p. 193). People may disagree with this observation, but differences of opinion will not reduce the significance of the book.

The Wade-Giles system is used throughout in transliteration. In view of the increasing popularity of the *pinyin* system, a conversion table of names (for example, Liu Shao-ch'i to Liu Shaoqi and Tientsin to Tianjin) might have been provided for people struggling with both systems. Similarly, a glossary in Chinese and some annotation on certain primary sources would have further enhanced the value of this admirable monograph.

TA-LING LEE

Southern Connecticut State College

JOHN D. PIERSON. *Tokutomi Sohō, 1863–1957: A Journalist for Modern Japan*. Princeton: Princeton University Press. 1980. Pp. viii, 453. \$30.00.

Tokutomi Ichirō began his career as a little Fukuzawa, committed to a cosmopolitan vision of a democratic and prosperous future Japan; he ended it a discredited but unrepentant nationalist, convinced that Japan had had to pursue its own special destiny in a hostile world. John D. Pierson's excellent biography does not defend or condemn Tokutomi for his inconstancy but performs the more useful task of making this transformation comprehensible.

More than half the book is devoted to Tokutomi's early career, the years of his sudden rise as spokesman for the young progressive intelligentsia of Meiji Japan. The author gives a well-rounded picture of the influences that shaped Tokutomi's early attachment to the optimistic pieties of mid-Victorian Anglo-American social thought—his birth into an established provincial *gōshi* family, the activism of his bookish father, the taunts of schoolboys from ex-samurai families, contact with the Christianity of the redoubtable Captain James, and an education in Western liberalism at Dōshisha.

Equally enlightening is the book's treatment of the collapse of Tokutomi's early optimism and his conversion to a darker and more pessimistic concept of Japan's position in the world. Pierson gives a more complex explanation of this change of heart than one finds either in Tokutomi's autobiography or in Kenneth Pyle's provocative monograph on intellectual currents of the Meiji twenties. He suggests that Tokutomi's increasing disillusion with the workings of domestic politics played as large a role in the change as the shock of the triple intervention did.

Pierson moves rather quickly over the last sixty years of Tokutomi's life, when he was a commercial success but had lost influence among the intelligentsia. Perhaps this is justified. Tokutomi had very little new to say in his later years. The main flaw of this section is that it provides too much historical context, most of it familiar, and too little analytical comment on Tokutomi's later writings, especially his monumental history of Tokugawa Japan.

Nevertheless, this is a fine piece of work, based on careful and exhaustive research, including much unpublished material. It is more than the usual pallid intellectual biography. We learn not only what Tokutomi wrote or thought, but what his marriage was like, how he treated his children, and what kind of businessman he was. It is richly detailed, exploring everything from Tokutomi's lineage to the economics of Meiji publishing to place him better in the context of his times. The only element lacking is a more complete portrait of Tokutomi's intellectual allies and colleagues. Pierson proposes no new paradigms for the study of modern Japanese intellectual history, but he has provided us with one of the best and most complete biographies in the field.

PETER DUUS

Stanford University

DUNCAN B. FORRESTER. *Caste and Christianity: Attitudes and Policies on Caste of Anglo-Saxon Protestant Missions in India*. (London Studies on South Asia, number 1.) London: Curzon Press or Humanities Press, Atlantic Highlands, N.J. 1980. Pp. viii, 227. \$13.75.

When two Indians, preparatory to their baptism, ate with the Baptists at Serampore, Bengal late in 1800, William Ward noted in his journal: "Thus the door of faith is opened to the Hindoos—Who shall shut it? Thus the chain of cast is broken—Who shall mend it?" From the beginnings of modern Protestant missionary activity in South Asia one of the major goals of evangelicals such as Ward has been the elimination of caste restrictions. Besides being a major obstacle to conversion, caste, in their eyes, retarded the development of education and all stages of progress.

In the first systematic coverage of this important subject, Duncan B. Forrester discusses the relationship of caste to Hinduism and the egalitarian side of Christianity. He then examines "critically the understanding and assessment of caste as it developed among Anglo-Saxon Protestant missionaries and Indian Christians associated with these particular missions from the late eighteenth century to recent times" (pp. 5-6).

The size of the book necessarily limits Forrester's treatment of any particular aspect of the topic. Even so, I feel it would have benefited from reorganization and more careful editing. For instance, less space should have been given to the conversion of the Paravar (Fisherman) caste in extreme southern India (discussed at least four times) and more to the formative years of the early nineteenth century—the years during which Ward, architect of the policies of the Serampore Mission and an ardent egalitarian, led Protestant missionary opposition to the caste system.

Forrester writes that "the most significant achievement of the Protestant critique of caste was undoubtedly its major contribution towards a radical transformation of educated opinion in India," but he does not argue this adequately. Chapter 8 ("Hindu Response to the Missionary Attack on Caste") largely consists of a summary of the thought on the subject of various Indian leaders, including Gandhi, rather than an analysis of, among other things, the ways in which these men might have been influenced by the Christian attack on caste.

Formerly a lecturer at Madras Christian College in Tambaram and now Professor of Christian Ethics and Practical Theology at the University of Edinburgh, the author based this "preliminary study" (p. 202) on a serviceable but not extensive examination of the relevant sources. An expanded version will require use of missionary records outside of the United Kingdom—American Protestants are sometimes discussed—as well as many relevant collections of material within Britain. In the meantime, Forrester's pioneering effort, published as the first in a series of London Studies on South Asia, will be of value to all students of modern India.

E. DANIEL POTTS
Monash University

KENNETH BALLHATCHET. *Race, Sex and Class under the Raj: Imperial Attitudes and Policies and their Critics, 1793-1905*. New York: St. Martin's Press. 1980. Pp. viii, 199. \$18.50.

Much recent British historiography on post-eighteenth-century South Asia, in its concentration on regional political developments and on Indian col-

laboration, has downplayed the imperial nature of British rule. This study, by Kenneth Ballhatchet of the University of London, does not fit into these molds. Instead, it focuses on imperial attitudes by exploring the most sensitive and emotionally charged issues in the entire imperial relationship, those of sex, race, and class. While the latter themes are not new to scholarship on British India, the study of sexual attitudes and practices certainly is. In this respect, and in the interrelationships that he draws between sex, race, and class, Ballhatchet's is a pioneering study.

The first half of the book consists of a detailed examination of the system of virtual state-regulated prostitution maintained by the British in their military cantonments and to a lesser extent in the large port cities of India. From the senior official perspective such regulation was necessary because young British soldiers, being of the lower classes, could not afford to support wives and families. They lacked the "high moral standard required" (p. 10) for sexual continence and might otherwise have turned to homosexuality or masturbation. The rationale for the official registration and medical treatment of Indian prostitutes was that it protected the British servicemen from the ravages of uncontrolled venereal disease. Working closely from official documents, Ballhatchet examines the early nineteenth-century origins of the system, details recurring discussions over its effectiveness, documents the success of the moral reform party in Britain in forcing the reluctant raj effectively to abandon the system in the early 1890s, and demonstrates the alacrity with which they restored it in 1897 after venereal disease among the troops had reached an alarming 52 percent.

The second part of the book focuses on official efforts to bolster British authority by maintaining an appropriate "social distance" between themselves and the subject race. Their most odious problem in this connection, namely that of lower-class European women who operated as prostitutes or barmaids in the port cities, is examined in an interesting chapter on "Low Life and Racial Prestige." Another chapter deals chiefly with district officials in Burma who did not share the racist mores of their superiors and defied censure by taking Burmese wives or mistresses. Attitudes to other groups that threatened "social distance" are also examined, namely the racially mixed Eurasians, who were increasingly ostracized; missionaries, who sometimes developed close ties with Indians; and princes, who might occasionally marry Europeans.

Source materials obviously present a major problem in this type of study. Such sensitive subject matter was automatically excluded from most private correspondence, and official sources, on which the study basically relies, deal mainly with flash points sparked by public exposure or with more no-

torious individual cases. This may result in distortions that the author seemingly overlooks. Were the official elite, for example, so sexually continent as he implies? On the whole, however, Ballhatchet handles the limitations of his sources well and generalizes judiciously and extensively from the particular. This is a study that is certain to stimulate further research.

EDWARD C. MOULTON
University of Manitoba

SARVEPALLI GOPAL. *Jawaharlal Nehru: A Biography*. Volume 2, 1947–1956. Cambridge: Harvard University Press. 1979. Pp. 346. \$20.00.

It is apparent in the second of Sarvepalli Gopal's three-volume life that Nehru devoted more attention to foreign affairs than to domestic matters in the first nine years of India's independence. Why would the prime minister look abroad when the problems of the refugees from Pakistan, economic development, the institutionalization of political democracy, and violent agitation for regional autonomy all required focused leadership? Part of the answer is that Nehru found his initiatives at home blunted by conservative cabinet colleagues and the constitutional decentralization of power. Foreign affairs offered greater scope for the exercise of Nehru's moralism and his ambition to thrust India into international prominence. Moreover, a consistent object of Nehru's foreign policy was to protect India's autonomy by trying to keep Asia out of the Cold War. Pakistan's decision to join Western-dominated military pacts seemed to Nehru a direct threat to Indian security and Asian peace. In Nehru's view, the preservation of a large bloc of nonaligned Asian and African countries reduced the likelihood of world war between the Communist and non-Communist alliances. And because Dulles and the U.S. were organizing military pacts in the 1950s when, after the Geneva and Bandung conferences, both the USSR and China seemed less bellicose, Nehru had come to view the West as a greater threat to peace than the USSR. This helps to explain why India voted in the United Nations to condemn the Suez invasion but abstained on the vote on the Soviet Union's suppression of the Hungarian revolt.

One of the many virtues of Gopal's book is that it draws heavily on Nehru's private papers to show the inner logic of what has often appeared to non-Indians as inconsistent or indecisive policy. The actions of the Nehru government at home and abroad have been more completely described elsewhere, but no one has given as full a picture of Nehru's own complex, rambling, reflective thought. The treatment is subtle, sympathetic, and sometimes critical. Nehru's failure to press for clarification of

China's border claims is seen as a major error of judgment. His tendency to make all major decisions himself even though this undermined his effort to establish the cabinet as a major democratic institution, his tardy recognition of the consequences of population growth, and his indecisive, drifting treatment of the demands for the creation of new states based on linguistic boundaries are all criticized.

There is little in Gopal's treatment to cause doubt about Nehru's basic seriousness and decency or his lack of pettiness and vindictiveness. When the chief ministers of the states trampled civil liberties, Nehru scolded them. He was repelled by totalitarianism—communist, fascist, or Hindu. He repeatedly reminded his colleagues about the need to protect the rights of Muslims, and he was appalled by the lack of similar concern among some of the highest officers in government, including the president (Prasad), the head of the Congress Party (Tandon), the deputy prime minister (Patel), and the chief minister of the U.P. (Pant). He seemed to place higher value on popular participation in politics than on achieving specific goals, such as the redistribution of wealth.

This book will be more useful to specialists on foreign affairs and South Asia than to nonspecialists because it assumes the reader is familiar with the background of the people, events, and institutions discussed. It will disappoint those seeking insights into Nehru's relations with his daughter, Indira Gandhi, who made Nehru's papers available to the author. Gopal states that she was not consulted "seriously" on public issues in these years (p. 311), and she is otherwise mentioned but twice.

JOHN R. MCLANE
Northwestern University

D. J. M. TATE. *The Making of Modern South-East Asia*. Volume 2, *The Western Impact: Economic and Social Change*. New York: Oxford University Press. 1979. Pp. xiii, 618. \$47.00.

This is the second volume of a major three-volume project on the history of modern Southeast Asia. D. J. M. Tate is a teacher in Malaysia, and, as such, he has designed the work to serve as an introduction to modern Southeast Asia at the pre-university level. This may be so for Malaysia, where the students are already exposed to Southeast Asian history in considerable depth before they reach the undergraduate level. In the U.S., the three volumes will not only serve the purpose of additional reading in the upper-level courses on Southeast Asia, they could also be used as a reference by those preparing term papers. The extensive footnotes given at the end of each chapter should serve such serious students well.

The work is divided into three segments. The first volume, which was welcomed enthusiastically by the scholarly world, narrated the saga of how and why Western control was established by the various powers over the diverse peoples of the region. The present volume claims to deal with the impact of the Western industrial civilization on Southeast Asian society and the socioeconomic changes this brought about. In contrast to the first volume, which dealt with indigenous kingdoms as political units wherever this was possible, the second volume treats the new administrative units, which more or less conform to the present-day political entities, carved out by the Western colonial powers. The third volume, the author promises, will handle the sociopolitical consequences of Westernization, the impact of education and new cultural modes, and the evolution of a sense of modern nationalism. This is reassuring indeed, because despite its claim the present volume has very little of the social consequences of Western rule; the treatment is predominantly economic.

Of the six chapters, five are purely descriptive. Four of them deal with a colonial area—Dutch, British, French, and Spanish-American, in that order—with subdivisions corresponding to the nation-states of the present time. Malaysia has received far closer treatment than any other country, 160 pages out of the total text of 569, with an obvious “Malaysian bent” meant to have “more immediate bearing and interest for Malaysians.” The last chapter deals with Thailand, which remained independent at the expense of some territorial losses to the Western powers and with tremendously reduced authority to determine its own economic and fiscal policies. The only chapter that is analytical is the first chapter, entitled “Agents of Change.” Beginning with a promising discussion of the major revolutions in industrial production, communications, and distribution techniques, the author fails to provide the underpinning of the economic or social theories used by the various colonial powers to exploit and to justify their colonial hold. The economic structure they erected should have been analyzed in terms of varying benefits to the colonial power or the people, the social impact in the context of the various anthropological theories that so dramatically influenced the minds of the European politicians, administrators, and thinkers in the nineteenth century. The “agents of change” have consequently been viewed more in physical than in sociological and psychological dimensions. One hopes this will be at least partly mitigated in the third volume as a background to the challenge to colonial control in the form of nationalist movements.

On the whole, however, the volume provides an excellent survey of the economic developments of the colonial period. The author’s meticulous, pains-

taking approach is evident on every page of the book, which includes six tables, seven charts, and thirty-seven maps. The charts well illustrate production in agricultural and nonagricultural fields, road and railway development, and demographic distribution. The value of the volume would have been greatly enhanced by a bibliography. Perhaps, the author will provide an extensive and comprehensive bibliography for all three volumes.

D. R. SARDESAI
University of California,
Los Angeles

HEATHER SUTHERLAND. *The Making of a Bureaucratic Elite: The Colonial Transformation of the Javanese Priyayi*. (Southeast Asia Publication Series, number 2.) Exeter, N.H.: Heinemann Educational Books, for the Asian Studies Association of Australia. 1979. Pp. xx, 182. \$14.95.

If Dutch colonial administration in the East Indies had a single guiding principle it was to govern through native headmen. In Heather Sutherland’s book we have an excellent study of what this principle meant in practice over the past two centuries in Java. As with all such seemingly timeless fundamentals of administration, the course of time and inevitable change have worn away the veneer and exposed the baser materials of the substratum.

Before Europeans ever set foot in Java the native administration had shown brief periods of much idealized unity and singleness of power and longer periods of uncertainty, change, and disruption. Dutch authority was gradually vested during such a period of uncertainty beginning in the late seventeenth century. The Dutch governed through local headmen from the first; there was no alternative other than noninvolvement. These headmen, as contact points with the great faceless masses, were endowed with qualities and powers beyond that which they had previously enjoyed. After 1800, when European notions of governance came to be applied in Java, these chiefs were gradually structured into a sort of Europeanized bureaucracy, while simultaneously retaining those traditional qualities that made them leaders of the rural mass population. Dutch policy, however, also saw this bureaucracy as the stimulator of economic growth in the agricultural commodity export sector. The conflicts for the Javanese bureaucratic elite are obvious. Their dilemma in seeking a middle path between tradition and modernization could not be resolved. This book conveys the details of the various efforts at solution and the ultimate failure of all these efforts.

Scholarship on the Indonesian elite during the past thirty years has made great advances toward

providing an ever sharper and more sophisticated view of Indonesian social change. This study adds a new dimension to this scholarship by describing in greater detail than previously the traditional background against which the political, educational, technological, and religious changes of the twentieth century were set. The native chiefs, with all of their Europeanized bureaucratic qualities, came to represent what the Indonesian modernizing elite of the twentieth century came to regard as traditional. Early twentieth-century efforts toward modernization, which, among other things, tore lustily into the prerogatives of the Javanese bureaucracy, showed that Javanese society was more flexible and less tradition bound than many conservative Europeans and Indonesians had realized. Preservation of the colonial order through sterner police measures and a reimposition of tradition became the dominant response to change in the 1920s and 1930s. A few of the Javanese bureaucratic elite entertained hopes of assuming active leadership roles in the modernization and democratization of Indonesian society, but that was not to be. Colonial society had become too polarized, and decolonization, when it came to Indonesia after World War II, had no real grounds for occurring peacefully.

ROBERT VAN NIEL
University of Hawaii

COLIN NEWBURY. *Tahiti Nui: Change and Survival in French Polynesia, 1767-1945*. Honolulu: University Press of Hawaii. 1980. Pp. xvi, 380. \$25.00.

The economic interchanges that have characterized European contact with Tahiti form the principle emphasis of Colin Newbury's study. Consisting of ten thematic chapters, *Tahiti Nui* begins with the early period of simple barter and gift exchanges between Europeans and islanders. As the number of visits by European vessels increased, a small class of entrepreneurs began to function as intermediaries between ship and shore. Visitors to Tahiti were, during this time, valued for whatever artifacts they brought that could be readily assimilated into Tahitian society. Firearms, a valued commodity, were incorporated into island styles of warfare as competing chiefs began a period of intra-island wars for hegemony over Tahiti.

The evangelical movement brought another foreign constituency to Tahiti. The London Missionary Society optimistically attempted to transform Tahiti into a Polynesian Zion. It was, moreover, anxious to develop a peasant class inspired with the nobility of the work ethic. Missionary efforts to transform the island economy from semisubsistence agriculture and aquaculture to cash cropping and commerce were only partially successful. Internecine warfare, confusion over land tenure claims,

and depopulation frustrated missionary plans to stabilize a deteriorating situation in Tahiti. Furthermore, the missionaries failed to resolve problems of public order outside of their control and expertise.

French administrative policies in the early 1840s precipitated resistance movements by Tahitian patriot chiefs. During this period, Tahiti resembled more a military outpost than the commercial colony envisioned by the colonial administrators. The dualistic nature of French-Tahitian governance became increasingly difficult to manage by a succession of colonial governors. Problems of administrative expenses and land tenure disputes frustrated attempts to assimilate Tahiti into the francophone world. During the 1860s, the Euro-Polynesian *métis* emerged as an identifiable class, undertaking much of the economic activity in the islands. The formation of merchant and brokerage houses was an important development as island commerce soon passed into the hands of these more substantial entrepreneurs. Island institutions were transformed as Tahitian life became more metropolitan. The search for profitable staple crops was frustrated by problems in capitalization, cheap labor sources, and confusion in land transfers. The plantation economy as developed elsewhere in the Pacific never became an important feature in the Tahitian economy.

By the early 1900s, Tahiti began to resemble a heterogeneous municipality and a roadstead for shipping and commerce. While material prosperity was linked to such change, the Tahitians were urged, if not compelled, to forfeit communal life for more individualized and fixed identities as part of the assimilationist policies of government and business. Tahiti became an integral part of the French union as the last vestiges of outmoded Polynesian institutions were discarded by statute. Although assimilation did not result in any wholesale redistribution of power or substantive incorporation of the Tahitians into introduced administrative institutions, substantial modifications of these institutions had to be made to meet local needs or else they were discarded as unworkable.

The author's analysis of local market economies as a link to political power is his principle contribution. *Tahiti Nui* is a serious consideration of new ideas that may prove useful in explaining the development of tropical commerce in frontier areas of the Pacific and Africa where parallel conditions have occurred. The bibliographical references reflect the conscientiousness and thoroughness of Newbury's study and establish Tahiti as a serious case study in transitional developments resulting from multicultural contact over a prolonged period of time.

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RALPH M. WILTGEN. *The Founding of the Roman Catholic Church in Oceania, 1825 to 1850*. Canberra: Australian National University Press; distributed by Books Australia, Norwalk, Conn. 1979. Pp. xxii, 610. \$27.00.

With a wealth of detail from congregational archives, Ralph M. Wiltgen has constructed a chronicle of the administrative history of Catholic missionary expansion in the Pacific from the establishment of an apostolic prefecture for the Sandwich (Hawaiian) Islands in 1825 until the negotiations that relieved the Society of Mary of its posts in Micronesia and Melanesia in 1850. By that date the Marists and the Sacred Hearts congregation (Picpus) had spent a quarter of a century building up eight apostolic vicariates in the principal island groups. There were also newly founded colonial dioceses in Australia and New Zealand after 1842, and their early history has been usefully surveyed in five of the thirty-five chapters, which also include an original contribution on the prefecture devoted to the aborigines at Moreton Bay.

The bulk of the history deals with the origins of this immense ecclesiastical partition, the motives of the founders, and the mobilization of resources in French and Italian congregations. Within this framework Wiltgen concentrates on official intrigue surrounding the sponsorship of the Marist and Sacred Hearts missionaries, their divided jurisdiction, and their relationship with the authority for Catholic missionary enterprise within the Evangelization Congregation (*de Propaganda Fide*). The clash of personalities and policies, particularly between Father Jean-Claude Colin, founder of the Marists, and Bishop Pompallier, founder of the Catholic mission in New Zealand, is made into a central theme of the period between 1838 and 1846. The reasons for the creation of separate vicariates for central Oceania, Melanesia, and Micronesia are also traced with infinite patience to the politics of ecclesiastical influence as much as to practical difficulties over finance, transport, and supply in the mission field. Not the least merit of this long study, therefore, is to make clear the precise structure and lines of authority between the missionary orders and the officials of the Sacred congregation and to bring to light the important roles of the cardinals, particularly Mauro Cappellari, who continued his mission patronage as Pope Gregory XVI, and Cardinal Francsoni, who did his best to mediate between Colin and Pompallier.

Rather less attention is given to the relationships between congregations and the French state, apart from the interest taken by Louis-Philippe and his court. The theme is touched on in passing rather than explored in depth in the absence of material from French naval or colonial sources. Thus, the precise content of official plans for posts in Oceania

in 1839 is never made clear (pp. 217–18), and French support for the Akaroa settlement in New Zealand is indicated only from Marist records. Other trouble spots for French missionaries in Hawaii and Tahiti are mentioned very briefly, and the reasons for the earliest French occupation of New Caledonia or for the close cooperation between the French navy and the missionaries in the Marquesas group in 1843 are not discussed.

A third theme that Pacific historians will search for is the interaction of the Catholic founders with their potential converts. The death of Bishop Epalle in the Solomons is given a chapter, but the early setbacks in New Caledonia are narrated, rather than accounted for. If it is true, as Wiltgen claims on the ultimate page of this immense text, that "much good had been done" by the new missions, then perhaps some space might have been devoted to illustrating these improvements to island societies.

While welcoming this solidly documented history of official motives and maneuvers, therefore, one cannot help regretting that a chance has been lost to furnish us with a much-needed study of the Catholic church in action in the Pacific in the first half of the nineteenth century, if only because there is every evidence of careful scholarship and generous financial subsidy behind this work. The maps and facsimiles are apposite and finely reproduced. Notable omissions from the bibliography are the works of T. L. Buick, J. P. Faivre, and J. W. Davidson, whose studies might have assisted the author on points about French policy, Akaroa, and Captain Peter Dillon, who features so prominently in the early chapters. Strangely, R. P. Perbal's work on French missions and the state in the Pacific is not mentioned either. One hopes that Wiltgen will be encouraged to take this study beyond 1850 and supplement his unique knowledge of the congregations with material that would help evaluate their work, as well as account for their presence.

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URSULA M. L. BYGOTT. *With Pen and Tongue: The Jesuits in Australia, 1865–1939*. Carlton, Victoria: Melbourne University Press; distributed by ISBS, Forest Grove, Oreg. 1980. Pp. xvi, 423. \$37.50.

Recent works by such historians as T. L. Suttor and Patrick O'Farrell have added greatly to the quality of the available works on the history of Catholicism in Australia. Such authors have broken away from the essentially filiopietistic strain that dominated earlier efforts; they have managed, without laying violent hands on the faith, to consider objectively

the attempts of the largely hibernicized church to come to terms with the overwhelmingly liberal, secular, British, Protestant, and materialistic society in which it was placed.

Ursula M. L. Bygott's work is of narrower scope. Rather than dealing with the concerns of the Catholic church in Australian society, she has written of the work of the members of the Society of Jesus in Australian Catholicism. The author has made a major effort. Her research has been thorough and intense; her bibliography is lengthy and her footnotes overwhelming. There can be few members of the Jesuits who do not receive their measure of recognition in the work. Such characteristics are meritorious but not without their costs, for the work at times reads rather like an alumni magazine recounting the achievements of all the graduates and extolling their virtues. In general, Bygott's labors on this work would seem to be excellent preparation for the study in which she is currently engaged, a history of the University of Sydney.

The educational activities of the Jesuits brought an academic rigor and elitism to an Australian Catholicism that was overwhelmingly anti-intellectual and to an Australian society that was strongly egalitarian. Just as political activity and trade-union leadership became channels through which Catholics attained positions of political leadership in Australia, so did the Jesuit schools and later the Jesuit colleges at universities become the channels through which some Catholics reached professional levels in Australian life. Owing, however, to episcopal indifference if not outright hostility to such Jesuit concerns, the major Jesuit activities and achievements were chiefly confined to the states of Victoria and New South Wales, with the former having undoubted pride of place because of the hospitality and encouragement given by the formidable Archbishop Daniel Mannix.

Even in the state where the Jesuits had their greatest achievements, however, their labors were essentially highly traditional and derivative in character. Sound learning and some scholarship of a conventional variety rather than anything suggestive of intellectual creativity prevailed in the ranks of the Society. As Bygott justly points out, the work of the members of the order undoubtedly fostered some intellectual growth in a Catholic society, indeed in an Australian society, that was in many respects indifferent to such values. For this their efforts deserve attention and applause. But one could wish that the author had contented herself with recounting them more briefly and that she had given more attention to the woods and less to the trees.

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UNITED STATES

GLENN PORTER, editor. *Encyclopedia of American Economic History: Studies of the Principal Movements and Ideas*. In three volumes. New York: Charles Scribner's Sons. 1980. Pp. xii, 483; 487-893; 894-1,286.

The aim of the *Encyclopedia of American Economic History* is, in the words of its editor, Glenn Porter, to make "available in more or less cohesive form the views of many specialists on a number of aspects of the collective American economic experience as it is understood in the latter part of the 1970's" (p. vii).

The three volumes of the encyclopedia are divided into five parts. Part 1 consists of an essay on the historiography of American economic history by William Parker; part 2, of six chronological essays on American economic history. Part 3 is concerned with the framework of American economic growth; part 4, the institutional framework; and part 5, the social framework.

Porter's thoughtful preface defines the problems involved in interpreting American economic experience in a field that directly crosses the two academic disciplines of history and economics and touches on all the social sciences. Both the "old" economic history dominated by historians and the "new" economic history dominated by economists are represented by a cross-section of the fields' most distinguished practitioners.

The opening essay is an auspicious start. Parker, by temperament and inclination an old economic historian, confronts Parker, by training and association a new economic historian. The result is a perceptive, if idiosyncratic view, of the development of the field. The six chronological essays that follow give the general reader a feeling for the main lines of development and problems of the American economy from its European origins to the present, although the discipline orientation, emphasis, and prejudices of the authors make one sometimes wonder if they are all talking about the same country.

Part 3 consists of a series of excellent and, for the most part, authoritative essays by leading experts, such as Robert Gallman, on economic growth; Richard Easterlin, on population; Lance Davis, on savings and investment; and Nathan Rosenberg, on technology. For those seeking a ready source of reference and summary of the quantitative evidence, this section alone makes the encyclopedia worthwhile. While not all the essays in this section are of uniform quality, the average is high. I particularly liked Donald Adams's essay on wages and prices, and J. R. T. Hughes's essay on entrepreneurship, which is really about law and property rights.

The last two sections are much more of a mixed bag, both because of the topics chosen for inclusion

and because of the varied quality of the essays. Section 4 begins with an excellent essay by Harry Schreiber on law and political institutions. Other above-average essays in the section include Gerald Gunderson on slavery, Allen Bogue on land policy and sales, and Alfred Chandler on the rise and evolution of big business (really a summary of *The Visible Hand*). A number of essays in this section are notably weak in analytical content, including those on competition, advertising and public relations, regulatory agencies, and government management of the economy.

Two essays in the final section are particularly noteworthy. Alan Olmstead on the costs of economic growth provides an excellent essay on an issue that has been the subject of a great deal of heat and too little light. I only wish he had devoted more attention to the deficiencies of national income accounting as a framework—a framework that has provided far too much of a straitjacket on shaping our thinking about the evolution of the American economy. Claudia Golden has written an excellent original essay on war, a subject economic historians tend to neglect.

Each essay concludes with an annotated bibliography, and, in this respect, the variation in quality is striking. Some bibliographies, like that following William Parker's introductory essay, are mere perfunctory nods at the literature; others (more often than not historians are the authors) have taken that responsibility seriously and produced useful reference material for nonspecialists.

There are some curious omissions in the encyclopedia. There is no essay on the balance of payments, and the rather disappointing essay on foreign trade has only one indexed reference to the balance of payments. As a result, the important part played by shipping earnings, immigrants' remittances, specie flows, and international capital movements in the American experience is nowhere specifically the focus of attention, even though these subjects are discussed in the chronological essays and referred to elsewhere.

The interplay between government, law, property rights, and the economy is inadequately treated, given its importance. There is Harry Schreiber's excellent essay (and Jonathan Hughes's) but the topic should have been the central focus of a number of essays.

A. W. Coats writes his typically authoritative essay on economic thought but slights, in fact neglects, the most important issues for this encyclopedia: the kind of theory economic historians have applied to history, the promise and limitations inherent in that theory, and, therefore, the consequences in terms of the way we have interpreted our past. While these issues come up in Parker's es-

say, they surely deserve far greater attention. Neo-classical economic theory embodied in the new economic history has illuminated a number of aspects of the American experience; but the blinders it has imposed have prevented new economic historians from exploring many aspects of economic history that more traditional historians have correctly (if intuitively) felt were important. Our interpretation of the past is no better than the theory we employ to sort out, weigh, and combine the historical evidence into explanation. That theory has been woefully inadequate.

Encyclopedias at their best give us a summary of the state of the art. Despite the shortcomings I have alluded to, the overall results are probably as close to that objective as one could achieve, given the problems outlined in the preface.

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CAROL WEISBROD. *The Boundaries of Utopia*. New York: Pantheon. 1980. Pp. xxii, 297. \$15.95.

Two commonplace assumptions about communal societies center on the quarrelsome nature of their internal affairs and the hostility that they experienced from external sources. They fought among themselves, and they fought off their neighbors. In this interesting volume, Carol Weisbrod refutes that conventional wisdom with her examination of the lawsuits brought against four utopian societies by former members.

Nine cases involving the Mormons, the Shakers, the Harmonists, and Oneida serve as the basis for this study. We are drawn into an area where utopian definitions, individual interpretation, and societal norms about the true nature of a contract and a community came into conflict. In particular cases the conflict between the community and the surrounding society was drawn sharply as with the Mormons and polygamy, the Shakers and celibacy, Oneida and complex marriage. Although celibacy was considered odd, and complex marriage and polygamy immoral, the courts were slow to suppress the colonies and treated them within an American religious tradition that tolerated dissent, encouraged pluralism, and presumed voluntarism as a guiding principle. When presented with grievances by former members, the courts generally ruled against the plaintiffs in their demands for back wages, for property brought in, and in so doing upheld the contract rights of the associationists. To be sure, the colonists avoided litigation whenever possible and made every effort to maintain cordial relations with their neighbors, but the courts did sustain their continuing existence with a strong

commitment to religious freedom for the utopians.

Carol Weisbrod is at pains to limit her discussion to the legal merits of each case and to avoid generalizing from these few cases to larger considerations. But some generalizations do emerge from this study: that suppression by means of the legal apparatus of the state did not contribute to the decline of such groups and that the private law of contract enabled them to protect their vital communal interests. Their existence and continuance found support and sanction from the courts.

By limiting her study, Weisbrod is able to give a close reading to both the community response to defections and the limits of contract law. For the communities, such defections were apostasy and generated strong feelings from the remaining believers. The courts considered their public responsibility to set social limits but usually declined to act simply because the group was led by a prophet some believed a religious fraud. In a case concerning the Harmonists, Chief Justice John Gibson of the Pennsylvania Supreme Court wrote: "He who conscientiously declares an indifferent or absurd theory to be essential to salvation may be a fanatic, but he is not a cheat."

This book might have embraced a wider range of cases concerning the communities and would have profited from a discussion of the 1815 *White v. McBride* case when the Pleasant Hill Shakers instituted a suit against the deputy sheriff of Mercer County, Kentucky, after he had taken their property to satisfy fines against members who refused to attend military musters. The court upheld the Shakers.

This is a solid work suggestive for other researchers interested in an area where the boundaries of the law and alternative social practices meet and then diverge.

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CHARLES DEBENEDETTI. *The Peace Reform in American History*. Bloomington: Indiana University Press. 1980. Pp. xvii, 245. \$18.50.

For the first time since Merle Curti's pioneering *Peace or War* (1936), Charles DeBenedetti has attempted to survey the entire course of the "peace reform" in America from the colonial era to the present.

The result is an admirably concise, scrupulously encyclopedic account that achieves the author's goal of creating a "broad synthesis of existing scholarly writing." The synthesis, however, does not lead to a central thesis or thematic unity. It is difficult to imagine how this deficiency could have been averted. Certainly the historian is not obliged to

supply retrospectively a unity of outlook or continuity of program that his subjects failed to achieve.

Although he refers to "the" peace movement, DeBenedetti frequently confesses that diversity was the most salient characteristic of peace reform. Advocates of peace, as internationalist James T. Shotwell once remarked, held "divergent views concerning not only the way to get peace but the nature of peace itself." Given such internal differences, DeBenedetti concludes, "there should be no wonder as to the antiwar movement's limitations. What is surprising is that it organized and persevered."

Drawing upon recent monographs, including his own study of the 1920-35 era, DeBenedetti chronicles successive phases of antiwar advocacy. These are characterized in chapter titles as "Sectarian" (1607-1763), "Revolutionary" (1763-1815), "Humanitarian" (1815-65), "Cosmopolitan" (1865-1900), "Practical" (1900-19), "Necessary" (1919-41), "Subversive" (1941-61), and "Deferred" (1961-75). Such thumbnail designations define aspects of peace advocacy more than they do stages of a movement. DeBenedetti does not describe an accretion of insight or influence. Peace reform did not grow or develop. Rather, it lurched along reactively, successively embodied in a bewildering proliferation of predominantly short-lived organizations.

Recognizing the pervasiveness of factionalism among peace advocates, DeBenedetti seeks to explore the "fault lines" between internationalists, religious sectarians, and radical pacifists—lines that intermittently widened into divisive fissures. In assessing the fervor with which most peace reformers united behind efforts to bring the United States into the World Court, he explains convincingly how the very inconsequentiality of the proposal served to magnify its importance. Failure on such an insignificant issue threatened to expose the weakness of peace reform. The major interpretive contribution of *The Peace Reform*, however, lies in its terse and effective exploration of the complexities of peace activism since 1960.

The strengths of DeBenedetti's survey contribute, perhaps inescapably, to its weaknesses. Qualities of balance and brevity, combined with an emphasis on coverage, give the work a textbook quality. The occasional vibrancy of peace activism, the play of motives and passions, is often suffocated amidst the recital of endless organizational titles. A student new to the topic is likely to be as dismayed by the parade of organizational abbreviations as intrigued by the reformers' tactics and ideas.

DeBenedetti concludes that peace reform never became a mass movement. It was relegated to the status of a "minority reform" because it opposed the nation's "dominant power culture." Another reason may be the seeming distance of the peace issue from

everyday lives, a perception largely unaltered even by realization of the possibilities of nuclear annihilation. Except during war crises, few Americans have been persuaded to focus their attention with any continuity on an issue that has generally seemed too abstract and distant to evoke mass concern.

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SAMUEL WALKER. *Popular Justice: A History of American Criminal Justice*. New York: Oxford University Press. 1980. Pp. xiii, 287. \$12.95.

Criminal justice is a relatively new field of study among professional historians. Interest in this area dates only from the mid-1960s, when the turmoil of that decade encouraged a few scholars to examine some aspect of the system, most notably the police. Since then a growing number of historians have broadened our knowledge of various institutions within the rubric of criminal justice, but the field as a whole has lacked a coherent framework for analysis. In this book Samuel Walker offers a pioneering effort to provide such a framework.

Walker has three primary concerns. He seeks to examine changing patterns of criminal activity, relate those changes to the development of a criminal justice system, and establish the major themes in the administration of justice. Underlying the discussion of these concerns is the argument that popular influence over the criminal justice system is responsible for its strengths and weaknesses over time.

Proceeding chronologically, Walker describes the criminal justice system as it evolved from the colonial period to the present. He examines law enforcement, penal institutions, and reform ideas and their impact in some detail. The best analysis throughout the book deals with prisons: their origins, evolution, and cycles of reform. In general, Walker's analysis demonstrates the frustrations of good intentions foundering on the rocks of ill-conceived ideas, bureaucratic needs, and public indifference. His discussion of police reform is also well done. This is one of the best short discussions of the success, and shortcomings, of the movement to professionalize the police. Finally, his indictment of federal law enforcement's abuses in the twentieth century is both cogent and comprehensive.

Some topics are not as well handled, however. Walker attributes the development of a police subculture to the defeat of police unionism shortly after World War I. While certainly a contributing factor, this account fails to recognize the origins of that subculture in the working conditions of patrolmen well before 1900. Isolation on the beat, the violence

policemen encountered, and the lack of public sympathy for their problems all contributed to the emergence of a police subculture during the nineteenth century. On another topic Walker adopts too readily the analogy of big business as the model for understanding organized crime. Recent research, especially by Mark Haller, indicates that this analogy is suspect. Criminal entrepreneurs typically used multiple partnerships in various activities, and these associations do not appear to have been very conducive to vertical integration or the monopolistic tendencies implicit in the business analogy.

Finally, Walker inadequately examines changing patterns of criminal behavior. This is an area where historical research is still scarce, but there is enough data available to make some effort to summarize major trends as we now understand them. With some few exceptions, Walker does not grapple with this important topic. As a consequence the reader is left wondering whether police reform and correctional innovations addressed real problems adequately.

In general, though, this is an important attempt to integrate current research into a coherent framework. In a field where the gaps in the secondary literature are still so massive, he has performed a very useful service. This is a valuable introduction to a subject that is rapidly increasing in sophistication and importance.

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RICHARD DRINNON. *Facing West: The Metaphysics of Indian-Hating and Empire-Building*. Minneapolis: University of Minnesota Press. 1980. Pp. xx, 571. \$20.00.

Richard Drinnon's study analyzes the ideological and mythological structures that underlay historical rationales of westward expansion, from the colonial period to the era of the Vietnam War. The book builds on and extends work done in the myth-symbol tradition (my own *Regeneration Through Violence* and Smith's *Virgin Land*, among others), psychohistory (especially Rogin's *Father's and Children* and the seminal essay on "Liberal Society and the Indian Question"), and revisionist history ranging from the colonial period (Francis Jennings) to the contemporary.

Historians who automatically bridle at any one of these identifications will be doing themselves a disservice if they neglect this work. *Facing West* is a sophisticated and detailed interpretation of historical evidence.

Drinnon's object is not merely to interpret literary or quasi-literary productions, elucidating sym-

bol systems for purposes of critical interpretation, but to interpret the language of a complete range of documents, with particular emphasis on those that define the making of policy. Although his conclusions will, of necessity, be controversial, I find them persuasively argued. And even those who do not agree with his interpretation will profit from the wide-ranging research that lies behind the work, the admirable notes, and the bibliography.

The strongest and most original sections of the book are those that deal with the post-Civil War period. Here Drinnon goes beyond the areas covered by Slotkin and Rogin to deal with the transformation of the concept of expansion (and of Indian policy) in the industrializing and imperial periods. He is one of the few writers to see the linkage between the conservationist impulse of "Indiophiles," like Mary Austin and Helen Hunt Jackson, and the values of expansionists, like Fiske and Roosevelt. The connections he draws between the changing character of domestic racism, "Indian-hating," and imperialism are suggestive and open up important new perspectives for future inquiry. The character of his study inclines him to show how the path of influence ran from racism on the border to racism in the metropolis to imperialism abroad. It would be useful, I think, to show the reciprocal character of influence between the different areas of the culture, focusing on class issues within the metropolis during all phases of expansion, including the present quasi-imperial one. Drinnon suggests the importance of class in his analogy between policies and prejudices toward the Irish and those toward Indians and blacks; and class feeling was a strong and significant element in the advocacy of imperialism, and in general in the new definitions of "racial" character that were put forward after the Civil War. But the focus on expansion and events beyond the borders perhaps inevitably shifts the emphasis away from the centers of class conflict in factory and city.

The sections on Vietnam are excellent, although restricted primarily to documents reflecting the response of officials and soldiers to the war. Drinnon is good at showing how the tradition of "Indian-hating" developed in the nineteenth century continued to operate and to ramify into other myths and ideologies well into the twentieth. Again, more might have been done with the domestic scene—with, for example, the peculiar but still traditional "Indiophilism" of the counterculture and anti-war movements of the Vietnam years and the differing responses of social classes to the war and its myths. But as it stands the book is a major contribution to scholarship, rich in both documentation and in suggestions for future research, and an interpretation of the past that scholars will have to reckon with.

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CARL BRIDENBAUGH. *Jamestown, 1544-1699*. New York: Oxford University Press. 1980. Pp. xiv, 199. \$12.95.

Carl Bridenbaugh has included many quotations from seventeenth-century sources in this book that he has addressed to the "general reader." Indeed, the dust jacket proclaims that this account of Jamestown is based "almost exclusively on a fresh reading of the surviving original sources." The original sources cited in the end notes are, however, the printed materials that historians have long used—not the kinds of archival materials, especially local records, that recent scholars have found so useful.

Nevertheless, Bridenbaugh offers some novel hypotheses. The author argues that Opechancanough, who planned the all-out attack on the English in 1622, was taken from his home in the Chesapeake to Spain in 1561, at the age of sixteen or seventeen. There and in Mexico, where he was known as "Don Luis," he learned, among other things, European diplomatic skills that contributed to his ability to carry out a master plan of "forest diplomacy" by which he organized Indian opposition to the alien intruders. Until fuller documentation and further details appear in Bridenbaugh's promised book on the subject (scheduled for publication in 1981), the reader will have to suspend judgment as to whether this interpretation does in fact advance our understanding of the early history of Virginia.

One of the most famous events to occur in Jamestown was Bacon's Rebellion, or more properly according to Bridenbaugh, "Bacon's Uprising, Lawrence's Rebellion." The author remarks that "the revolt has been far too long polarized around 'the governor and the rebel'" (p. 89). (*The Governor and the Rebel* happens to be the title of Wilcomb Washburn's well-known monograph on the subject, otherwise unmentioned.) The interpretation advanced here is that this was a conflict to determine which element of the planter oligarchy would exercise power—aristocrats like Richard Lawrence who took seriously their responsibility to work for the public good (which is undefined here) in return for the privileges of rank or a clique of self-made planters, "bourgeois plutocrats," toward whom Governor Berkeley looked for support.

The underlying weakness of the book is the author's failure to integrate into his synthesis the findings of other scholars dealing with the history of the Chesapeake region, of which Jamestown was an important part. Had he done so, he might have placed his colorful quotations about life in Jamestown into a more meaningful perspective. For example, Governor Berkeley is quoted as attributing the economic difficulty of the colony to "the vicious ruinous plant of Tobacco," a statement (made in 1663) that could be better understood in the context of the price of Virginia's export staple at specific times—data that are available in studies made

by economic historians. Again, Bridenbaugh quotes the comments of contemporaries about the "seasoning" of new arrivals to the colony, a subject that could be better interpreted in the context of recent scholarship dealing with the impact of disease and climate upon health and mortality in the seventeenth-century Chesapeake.

It is often said that professional historians write too much for each other and not enough for the general public that so badly needs a sense of history. But the question raised by *Jamestown, 1544-1699* is whether a professional historian in the late twentieth century can write effective history for the lay reader directly from the sources (which turn out in this case to be only a small portion of the source materials relevant to his subject) without using in some way the large body of scholarly literature now available.

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BERNARD W. SHEEHAN. *Savagism and Civility: Indians and Englishmen in Colonial Virginia*. New York: Cambridge University Press. 1980. Pp. xi, 258. Cloth \$29.50, paper \$6.95.

Bernard W. Sheehan's *Savagism and Civility* is his second contribution to "intellectual history," his previous essay having been *Seeds of Extinction* (1973). In *Savagism and Civility* Sheehan takes the word "savagism" and plays with it and with the reader. In the "mythology" of "savagism," as Sheehan presents the concept, the European is dominated by the idea of the Indian as a savage (and himself as "civil") and spends most of his time in the New World grappling with the "threat of savagism" or the "problem of savagism" rather than with the Indian himself. Sheehan's "savagism" encompasses both the "noble savage" and the "ignoble savage" while his concept of European "civility" includes both the "wylde menn of myne owene nacione," as Ralph Lane described the fractious Roanoke colonists over whom he tried to exercise authority (p. 167), and more polished exemplars of European civilization.

Sheehan's essay is fluent and internally consistent, but there is no historical measure by which it can be judged. Whether the Indian is perceived as virtuous or brutish, rational or irrational, farmer or hunter, well-ordered in his government or without government, he confirms and illustrates Sheehan's formulation of the "problem"—that the Englishmen of the seventeenth century were "gripped by the savage dogma" (p. 99) and unable to perceive the reality of Indian existence (conveniently undefined). Only occasionally does Sheehan directly confront the facts that challenge the application of his thesis. Thus, overwhelming documentary evidence of the Virginia Indian leader Powhatan's

well-ordered government and effective dealings with the Jamestown colony draws from Sheehan merely the grudging admission that "the acknowledgment of such a being as a 'civill Savage' represented a monumental concession from an ideology that insisted upon the strictest differentiation between civil life and the ways of the savage" (p. 93). And only in a footnote does Sheehan attempt to answer the objection made by Karen Ordahl Kupperman that "the expectation of treachery" (one of Sheehan's arguments for English dependence upon the concept of savagism) was "deeply rooted in the Englishman's view of human relations" and was not especially attached to the conception of the savage: "The doctrine of savagism cannot be rendered meaningless to Indian-white relations because the English might have behaved as badly without it. Because they believed the Spanish or French to be treacherous for one set of reasons does not mean that they would interpret the supposed treachery of the American 'savages' in the same light" (p. 219).

Every aspect of Indian-white relations dealt with by Sheehan illustrates the "problem" of savagism: John Rolfe's marriage to Pocahontas, in which Rolfe is able to "surmount" the problem and consummate the marriage (p. 130); the snatching by European ship captains of unsuspecting Indians for sale as slaves, a practice that Sheehan asserts white men "assumed [to be] within their rights" (p. 132); trade, where "savagism led Englishmen to expect a trade in raw materials" and where "savagism obscured the significance of the natives' talent in trading the products of the economy for the goods offered by the English" (p. 137).

Sheehan's refusal to read the evidence for its obvious meaning rather than conformity to his thesis is best revealed in his discussion of the brutal murders of an Indian queen and her children in 1610 after their capture in one of Powhatan's villages. In an incident reminiscent of My Lai, in which several superior officers censured subordinate officers for sparing the captives, the Indians were eventually dispatched. After recounting the disagreements over whether the captives should be "wasted," Sheehan concludes: "To every Englishman involved it seemed altogether fitting that savages of all ages should be slaughtered" (p. 169).

A simple explanation of the facts of Indian-white interaction in seventeenth-century Virginia is not beyond the reach of the historian. No intervening "doctrine of savagism" is necessary to describe or to explain the facts. Obviously the Indians (both those opposed to and those who collaborated with the English) demonstrated characteristics that Europeans classified under the heading of "savagism" just as they revealed other characteristics that Europeans associated with European concepts of civility, as the terms "kings," "queens," "councils," "wisdom," "justice," applied to the Indians suggest. But

the labels attached to the Indians are not the facts of history. It is the historian's task to penetrate the labels and get at the facts. This Sheehan resolutely refuses to do. He is more content manipulating the language of the past and constructing his own meaning from that language than he is in searching for the reality behind the words.

WILCOMB E. WASHBURN
Smithsonian Institution

THAD W. TATE and DAVID L. AMMERMAN, editors. *The Chesapeake in the Seventeenth Century: Essays on Anglo-American Society*. (Original Essay Series.) Chapel Hill: University of North Carolina Press; for the Institute of Early American History and Culture, Williamsburg, Va. 1979. Pp. viii, 310. \$26.00.

Thad W. Tate's forty-page "The Seventeenth-Century Chesapeake and Its Modern Historians" is an encyclopedic introduction to the eight essays in this third volume of the Original Essays Series published by the Institute of Early American History and Culture. The collection is a partial record of a November 1974 conference on the seventeenth-century Chesapeake colonies. Two of the papers read at the conference have appeared elsewhere: Alden T. Vaughan on English attitudes and the Powhatan uprising of 1622 in the *William and Mary Quarterly* (35 [1978]: 57-84) and T. H. Breen on cultural values and social behavior in early Virginia in the *South Atlantic Quarterly* (78 [1979]: 342-60). The volume's best essay is a new one, Carville V. Earle's persuasive attribution of sickness and death at early Jamestown to typhoid fever, dysentery, and salt intoxication caused by seasonal variations in the flow of the James River and in the drinking water at the first capital. James Horn's new essay on servant immigration and Kevin P. Kelly's new essay on settlement patterns in Surrey County, Virginia; as well as Darrett B. and Anita H. Rutman on family and society in Middlesex County, Virginia; Lorena S. Walsh on Maryland marriages and family; and Lois Green Carr and Russell R. Menard on early Maryland freedmen share a quantified, demographic approach to social history. David W. Jordan and Carole Shammas ascribe the Chesapeake "Golden Age" in Maryland and Virginia to the appearance of native-born elites in the decades either side of 1700.

As Rhys Isaac observed at the conference, the papers do not establish an interpretative framework for seventeenth-century Chesapeake society. The assumptions in 1974 were that the world of the tobacco colonies was "fragile" and lacked social order or political stability and that "the significant transformation . . . began late in the century, eventuating by 1720 in the predominance of a native-born social and political leadership, a large-scale plantation

economy, and a labor system based on black slavery." It remains to be seen whether these are "important themes . . . emerging in the work on the Chesapeake" (p. viii) or governing assumptions mirrored in conclusions. Only Darrett and Anita Rutman question the "chaotic" orthodoxy about early American society and politics defined by Bernard Bailyn's 1959 essay on politics and social structure in Virginia and vigorously challenged in papers read at the American Historical Association's 1979 meeting. Jordan and Shammas use nativity as a conveniently measurable attribute, but it is only one small facet of the region's complex political culture, and stability is a more complicated concept than these essayists suppose. Change is not necessarily instability, and political and social stabilities do not always concur. Shammas's reading of Virginia politics relies too heavily on an incomplete understanding of one exceptional man, William Fitzhugh, and ignores both the Green Spring faction and the changed imperial presence after Bacon's Rebellion. For example, Shammas writes that while "one might well imagine that the alignments provoked by Bacon's Rebellion would still inform the structure of politics, the colonists instead were flinging the epithets of 'Whig' and 'Papist' at one another and using these mother country labels to define their own local disputes" (p. 279). In fact, the statement obscures both local and imperial context by neglecting to mention the Catholicism presumed of Governor Effingham and professed by Fitzhugh's law partner, George Brent, which for some Virginians was at issue.

The third volume in the Original Essays Series will not enjoy the shelf life of the first, James Morton Smith, editor, *Seventeenth-Century America* (1959), and, as reports of work in progress, several essays would have been more notable if they had appeared sooner. It is an important sampler for one of the most lively areas of inquiry in contemporary American historical scholarship.

JON KUKLA
Virginia State Library

CHRISTOPHER M. JEDREY. *The World of John Cleaveland: Family and Community in Eighteenth-Century New England*. New York: W. W. Norton. 1979. Pp. xiii, 234. \$15.95.

This book, a revision of Christopher M. Jedrey's doctoral dissertation at Harvard University, describes town life in eighteenth-century Ipswich, Massachusetts, as it revolved around the village pastor, John Cleaveland (1722-99). Like Robert Gross's *Minutemen and Their World*, the work represents an imaginative blend of social and cultural history that examines eighteenth-century Ipswich as

a case study in New England rural life. In contrast to earlier town histories, which extrapolated a model of overcrowding, scarcity, and social confrontation from population data alone, Jedrey combines a quantitative study of Ipswich society with Cleaveland's religious and political pronouncements to document an enduring strain of Puritan communalism that retained much from the seventeenth century and resisted the atomistic tendencies prevalent in urban centers such as Boston. Instead of dislocation and inflamed passions, he concludes that, at least as far as Ipswich was concerned, "stability and self-sufficiency were its outstanding characteristics down to the eve of the Revolution" (p. 62).

Much of Jedrey's analysis relies on Cleaveland's own words. The book begins with Cleaveland's youth in Canterbury, Connecticut, where he experienced the pangs of the New Birth in the Great Awakening and participated in a separation from the local Congregational church. The pietistic attitudes absorbed in Cleaveland's youth were, Jedrey argues, not revolutionary, but part of the long-standing New England tradition of lay congregationalism and voluntary clerical maintenance. Lay assertiveness in the form of separations was not economically motivated (the Separates cannot be distinguished by their relative wealth or poverty) but instead represented an atavistic return to lay control and congregational ordination. These same traditions followed Cleaveland to Yale College where, with fellow rural ministerial students, he soon ran afoul of the college authorities. Like several of his classmates, Cleaveland was expelled from the school prior to completing his studies and was forced to seek clerical employment without the support of the established clergy.

From New Haven Jedrey follows Cleaveland's career first to Boston, and then to the village of Ipswich where he ministered to a Separate congregation sympathetic to his sense of lay-invested church authority. Throughout his long and successful career in Ipswich, Cleaveland never lost the convictions of his youth and would apply them to a broad range of religious and political contexts.

Like many rural pastors Cleaveland divided his time between preaching and farming, and in the acquisition and distribution of his property he evidenced patterns similar to the surrounding families. Despite diminished resources and rising population, fathers retained a strong measure of social control and stability through a careful allocation of land and capital. Through an extensive survey of Ipswich probate records, deeds, and tax lists, Jedrey demonstrates how Ipswich farmers supplied all of their children with some material legacy by practicing a careful "accommodation between the poles of impartible inheritance and equal divisions

of . . . real estate" (p. 81). The result was to minimize the dislocation and confrontation posed by overcrowding.

During the 1760s and 1770s Cleaveland demonstrated that he could be as skilled a political polemicist as a preacher. Here too he resembled many of his clerical peers who, in rural settings, found themselves best equipped to articulate colonial grievances against the mother country. Writing under the pseudonym *Johannes in Ermo* (John in the Wilderness), Cleaveland eagerly entered the political arena and presented scathing indictments of British oppression together with reasoned statements of the right of resistance to civil tyranny. Like his countrymen, his attitudes toward British policies were shaped by the Puritan preaching tradition that reflexively "brought forth a [political] response from rural New England based on the historical understanding of God's dealing with his 'New English Israel'" (p. 95). Unfortunately, Jedrey does not go on to explore the extent to which this essentially biblical understanding took precedence over the commonwealthman tradition in rural settings. Were New England farmers motivated by neo-classical statements of republican ideology or by a Hebraic self-understanding as the New Israel?

A full series of quantitative data on population growth, distribution of wealth, literacy, and inheritance patterns in Ipswich society support Jedrey's narrative and provide a starting point for more general studies of eighteenth-century Massachusetts culture and society. This is a valuable monograph that ought to be consulted by all historians concerned with questions of continuity and change in provincial New England society.

HARRY S. STOUT
University of Connecticut

LEOPOLD S. LAUNITZ-SCHÜRER, JR. *Loyal Whigs and Revolutionaries: The Making of the Revolution in New York, 1765-1776*. New York: New York University Press. 1980. Pp. xiv, 225. \$17.50.

The principal theme of this latest inquiry into "the making of the Revolution in New York" is the interplay of the domestic political rivalry of the De Lancey and Livingston factions with the imperial crisis, "the significance of which," the author baldly asserts, "historians have not yet recognized" (p. x). As it happens, a number of historians have written on the subject in recent years, particularly Roger Champagne, to whose findings and point of view this book bears a pale resemblance. Champagne takes exception to Carl Becker's classic exposition of factionalism in revolutionary New York as a manifestation of popular unrest by suggesting that such factionalism was no more than a cynical ex-

plotation of the quarrel with the mother country by the patrician politicians of both factions. Leopold S. Launitz-Schürer, Jr., allows the same priority to family politics in determining the development of the revolutionary movement but is more inclined to regard the Livingston and De Lancey politicians as sincere Whigs and themselves the victims of circumstances. "If the accidents of political fortune led the De Lanceys to loyalism," he offers in conclusion, "so the same accidental workings of events led to the Livingston triumph" (p. 197). Launitz-Schürer is here following to its logical conclusion the argument of some recent writers that there is little to distinguish the Whig from the Tory ideologically. What is left then but chance and the vagaries of temperament to account for the decisions made by these gentlemen?

Launitz-Schürer does indeed opt for "personality" as a way of further explaining the Whig-Tory schism, this time falling back on Pauline Maier's suggestion that such explanations might best be found "through comparative biography." It seems to me not much worthwhile dealing with such ahistorical schema, except to say that in the case of this book the results are in any event puerile, ("But the Livingstons were worthy opponents, whose personalities were also very formidable" [p. 171]).

If the author indulges in flabby conceptualization, he transgresses more seriously in his use of the evidence. He alleges, for example, that Alexander McDougall's broadside, "To the Betrayed Inhabitants . . . of New York," was nothing less than "an attack on the whole of the colony's elite," which is to construe that text beyond all reason (p. 90). Or in another instance he converts Isaac Sears and John Lamb to "active Presbyterians," when in fact what evidence there is suggests that they were Anglicans (p. 34). On another detail of consequence, Launitz-Schürer errs in the dating of a meeting at which Sears urged the Liberty Boys to support the candidacy of James De Lancey (pp. 36-37). These are not casual misconstructions, but very much of a piece with a text that is continually slippery in its assertions. All and all, I would not care to recommend this book to graduate students as a faithful narrative account of New York in this period of revolutionary crisis.

BERNARD FRIEDMAN
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REGINALD C. STUART. *The Half-way Pacifist: Thomas Jefferson's View of War*. Buffalo: University of Toronto Press. 1978. Pp. x, 93. \$10.00.

Reginald C. Stuart sketches Thomas Jefferson's assumptions on war and its roots in human nature in crisp, effective prose. Stuart finds Jefferson a man of the Enlightenment in his view of war as one of sev-

eral instruments for diplomacy, always to be used in a limited way and never to be employed where peaceful means might succeed. While these basic views never changed, circumstances caused Jefferson to stretch them, especially where frontier warfare and territorial gains were involved. Most of Stuart's book follows Jefferson's career chronologically, emphasizing moments when Jefferson either advocated force or thought it especially important to avoid it. Stuart's treatment is intelligent, and his conclusions sound. They disagree with the title, however: no man who supported two wars against Britain, waged a war against the Barbary states, threatened war at various times against both Spain and France, and accepted war as a frequent necessity in dealing with American Indians should be described as any sort of pacifist.

Scholars who have read widely on Jefferson and his times will find the material directly concerning Jefferson quite familiar but will benefit from the fresh context of modern theory concerning the role of war in international relations. Conversely, students of war and society with only a casual prior knowledge of Jefferson will find a useful summary of his thoughts and deeds respecting war that otherwise could be found only by reading several long biographies and diplomatic histories. Stuart especially has this audience in mind when he concludes that Jefferson and his colleagues definitely did not enter into wars as crusades; the Wilsonian spirit may have had roots in the Founding Fathers but was actually foreign to their thought.

A major conclusion should be qualified. Stuart argues that Jefferson's old-fashioned Enlightened ideas blinded him to the changing nature of European warfare: "In Europe the crusading zeal aroused by the revolutionary wars shattered the edifice of limited war; in America the structure remained intact in the mind of the *Secretary of State*" (p. 25, my italics). Granted that Jefferson and Madison seriously misjudged the international situation from 1806 onward, there was no clear pattern of French expansionism or European response to suggest modern total warfare in the 1790s. While Jefferson was secretary of state (1790-93) the wars of the French Revolution were clearly for limited objectives, and were so perceived in Europe.

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RICHARD M. ROLLINS. *The Long Journey of Noah Webster*. Philadelphia: University of Pennsylvania Press. 1980. Pp. xi, 195. \$16.00.

Historians who have heard exaggerated political views all their lives, known people who vowed they would "move to Canada" if so-and-so were elected president and all that sort of nonsense, will pay no

attention to these ravings but will write books and apparently think that Americans in 1800 or 1850 were totally different. In the rush to write "new" biographies and histories full of condemnation for elitists, aristocrats, and racists (who did not know they were racists), our younger historians need to occasionally look around and reflect on their own experience before they start pounding their typewriters. Then they would not speak of the 1790s as a time when "no one trusted his political opponent. . . . Toleration of divergent perspectives did not exist" (p. 91). Was our Revolution of 1800 a bloodbath?

Take away the psychological jargon, the startling full-page blurb for "a legendary unpublished manuscript" (p. 73), and the numerous "as——has pointed out" deferences, and Richard M. Rollins's work has considerable worth as an effort to show the forces working in (and at work on) Noah Webster. All the pain of adolescence and poverty is spelled out, the shaping of a typical New England mind is clarified, and the mixture of self-doubt and ego is revealed. But we are overwhelmed with the language of the psychiatrist's couch (frustrations, anxiety, alienation, anger), and when we read that Webster's "father and family had bluntly rejected him" (p. 19) the impact is lost.

Much more could have been made of Webster's drive to reshape education forces in America and to assist in the building of "a truly utopian form of society" (p. 34). The enormous influence of his little blue-backed speller, with a first printing of 5,000 copies, is almost lost in the shuffle as we see a rather pitiful Webster intruding on strangers and making a book promotion tour in 1785. Granted he was "pushy," but maybe he had something important to push beyond his own self-esteem. Granted that Webster was an etymological babe-in-the-woods, this reviewer is not convinced that Webster's work and resulting dictionary ought to be considered merely as "the product of a complex interaction of an individual and the society in which he lived" (p. 125). If it is simplistic to call Webster's dictionary "a nationalist tract" (p. 127), then why did it come onto the scene when it did and sweep away all other similar products? Charles A. Beard, Oscar Handlin, Merle Curti, John D. Hicks—you guys sure missed the point!

There is much of value in the book. The author has overreached himself but deserves praise for his wide research and genuine efforts to provide a broader understanding of the Federalist mind as exemplified in Webster's life.

ROBERT A. RUTLAND
University of Virginia

JOAN JACOBS BRUMBERG. *Mission for Life: The Story of the Family of Adoniram Judson, the Dramatic Events of the First American Foreign Mission, and the Course of Evan-*

gelical Religion in the Nineteenth Century. New York: Free Press. 1980. Pp. xvi, 302. \$12.95.

This is a lively exercise in collective biography, focusing on the lives of selected members of a pioneer American missionary family. Adoniram Judson, who established a mission station in Burma in 1813 after he and his first wife were converted to antipedobaptism while sailing to India under the auspices of the Congregationalist-sponsored American Board of Commissioners for Foreign Missions, became an enduring hero and inspiration to his fellow Baptists and other evangelical Protestants. As Joan Jacobs Brumberg writes, he "made himself a metaphor for the whole of evangelical culture" (p. 46). In order to explore the nature of nineteenth-century American religious life and thought Brumberg has undertaken an ambitious study of Judson himself, his three successive wives, and three of their children—a daughter who became a spiritualist lecturer, a stepson who played a minor role in the promotion of the social gospel, and finally his youngest son, who founded and conducted the affairs of the Judson Memorial Church in New York city with the financial backing of John D. Rockefeller.

Although she pays some attention to the work and the personal sufferings of the great missionary patriarch and his wives in Burma, especially as portrayed in the tremendous outpouring of hagiography their lives inspired, the author is less concerned with the enterprise of foreign missions itself than with such matters as the influence of the religious press and its links with popular literature, the socialization of evangelical youth, and the peculiar role of women in missionary affairs. Asserting that "women were at the heart of American evangelical religious culture" (p. 79), she documents the emergence of a "religious feminism" that promoted egalitarianism along with a form of female leadership in Protestant churches without coming into direct conflict with the prevailing "cult of true womanhood." The latter third of the book is less successful in attempting to trace the evangelical impulse that lay behind foreign missions as it flowed in new channels when a later generation tried to reconcile its religious heritage with modern science and an urbanizing, industrializing society. The narrative occasionally bogs down in superfluous details and fails to develop a unifying theme broad enough to relate the lives and careers of the second generation to the first. But the work breaks new ground in religious biography and should encourage others to enter the field. If the Judsons do not quite rival the Beechers as a major force in American Protestantism, their lives have been shown to shed important light on the development of evangelical culture in nineteenth-century America.

CLIFTON J. PHILLIPS
DePauw University

JAMES T. SCHLEIFER. *The Making of Tocqueville's Democracy in America*. Chapel Hill: University of North Carolina Press. 1980. Pp. xxv, 387. \$26.00.

The first part of Tocqueville's *Democracy in America* was published in 1835. On June 12, 1837, John Quincy Adams wrote to Tocqueville that he had just read the great book and found it vastly superior to all previous French commentaries on the United States. "All of them," he added, "have remarked what was on the surface of the soil and of social life. Some of them have surveyed the country with favorable, and some with adverse partiality; no one I think with so wide a compass or so deep a penetration as yours." James T. Schleifer's indispensable reconstruction of the writing of the *Democracy* measurably advances our appreciation of how Tocqueville fashioned his illustrious book.

Following the path first so brilliantly cut out by George W. Pierson in his magisterial *Tocqueville and Beaumont in America* (1938), Schleifer has explicated the text of the *Democracy* by elegantly relating Tocqueville's notes, correspondence, and drafts for the *Democracy* to the work as it finally appeared. The Yale Tocqueville Manuscript Collection, so carefully built up by Pierson, has found in Schleifer a scholar equal to the task of tracing step by step the writing of the *Democracy*.

Schleifer does not attempt to elucidate every chapter or theme in the *Democracy* but limits himself to issues he judges central to Tocqueville's intention. He examines Tocqueville's discussion of the distinct circumstances of the American environment, the institutional character of the republic, the problematic relationship between centralization and democratic polity, the possibility of democratic despotism, the tyranny of the majority, egoism and individualism, and finally the varieties of Tocqueville's conception of *démocratie*.

Every reader of Schleifer's book will find material and comment that is particularly enlightening to his own Tocqueville. This reader was especially instructed by Schleifer's accenting of Tocqueville's insistence that he was writing in and about an "*époque de transition*," that he considered his work as belonging to the genre of "*philosophico-politique*," and by Schleifer's bold effort to state "What Tocqueville Meant by *Démocratie*."

This penultimate section of his exegesis does raise some problems. Throughout his analysis Schleifer pays passing attention to Tocqueville's response to what was happening in the first decade of the July Monarchy. Schleifer's command of this period is less than thorough. The result is that Tocqueville's concern for the future of democracy in France, his understanding of his time as one of transition, is inadequately related to the era in which he wrote. What he thought about *démocratie* must be seen in

the context of his contemporaries' encounter with the promise and expectation of a democratic society. The merit of Schleifer's book will be enhanced by reflection on André-Jean Tudesq's *La Démocratie en France depuis 1815* (1971). Tudesq's discussion of the significance given to the words "*démocratie, démocratique, démocrate*," and their juridical, sociological, and psychological connotation in different historical moments and "philosophical options," places Tocqueville within his milieu. This analysis does not subtract from Tocqueville's originality but adds to the universality of his accomplishment, which was singular in expression but part of a whole generation's discourse. Tocqueville did wish to stand alone. He was not, however, the last man in Europe's aristocratic history, nor the only troubled heir of the democratic revolution. To set him apart prohibits a more comprehensive knowledge of the movement he sought to understand and influence.

EDWARD T. GARGAN
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BARBARA NOVAK. *Nature and Culture: American Landscape and Painting, 1825-1875*. New York: Oxford University Press. 1980. Pp. xi, 323. \$35.00.

Barbara Novak's book, *Nature and Culture*, is in the tradition of art historical works that seem to be moving away from straight aesthetic analysis and toward a broad social view. This approach was popularized immediately after World War II in Oliver Larkin's Pulitzer Prize work, *Art and Life in America* and, more recently, in Joshua Taylor's books, *America as Art* and *The Fine Arts in America*. *Nature and Culture* attempts to bring together ideas in the history of American philosophy, science, literature, religion, and technology with the author's own expert view of American landscape painting of the period 1825-75.

Divided into four sections, *Nature and Culture* begins with the literary themes of nature, God, primordial wilderness, and the rural ideal as defined by Perry Miller, Leo Marx, Henry Nash Smith, R. W. B. Lewis, and others. It then moves to new ideas in scientific fields, particularly botany and geology, as researched by A. Hunter Dupree, Edward Lurie, Arthur O. Lovejoy, and Nathan Reingold. Next, the western explorations that William H. Goetzmann has treated are reviewed; and finally the American artists' experiences abroad are noted as well as the intellectual turmoil that these encounters created as reported by Henry James, Van Wyck Brooks, and Roger B. Stein. In all four sections, Novak tries to show a close relationship between landscape art and pervading cultural ideas

and experiences. Emerson, Thoreau, Tuckerman, and Whitman are heard repeatedly, and so are the landscape painters themselves, Thomas Cole, Albert Bierstadt, Asher Brown Durand, Frederic Edwin Church, Jasper F. Cropsey, Worthington Whittredge, Martin Johnson Heade, and others. *Nature and Culture* attempts to give the reader a sense of the intellectual and moral climate in which nineteenth-century landscape artists worked and by exploring whenever possible the reciprocal action between a culture that produced landscape art and the landscape artists' responses to that culture.

The broad concept of the work is alluring. The landscape artist touches diverse fields of inquiry and becomes a kind of bridge across a number of intellectual disciplines. Unfortunately, in my opinion, the book does not begin to reach its goal. Despite a few sparkling chapters on the meaning of clouds and plants and axes and railroad trains in landscape art, the work lacks unity. Six of the ten chapters had been published earlier at various times, in whole or in part, and this may contribute to the unevenness of the book. However, the basic problem is that it has no underlying theme. The author at one point notes, "The age was not simple. The artists, as part of the age, revealed in their aesthetic the contradictions in their society between science and art, between empiricism and the ideal, between analysis and synthesis, between technology and nature." If this were the theme of the book, there would be an organizing principle. Rather, the reader is given a series of essays, varying in quality, on different themes.

There is another problem with *Nature and Culture*. Only a fraction of the landscape artists who worked in the nineteenth century are discussed and, often, only one or two of them are used to suggest a relationship between a particular literary, scientific, or philosophic idea and painting. What applies to Jasper F. Cropsey may be totally irrelevant to Asher B. Durand, and even if an idea does relate to these two, it may not be pertinent to other landscape artists. In brief, the attempts to generalize lead repeatedly to imprecision. The intellectual leaps from individual artists to general groups such as "early-nineteenth-century America" or "society" or "culture" require too much faith.

On the other hand, *Nature and Culture* has much to recommend it. The author shows a keen ability to summarize the writing of others and present their ideas vividly and succinctly. For fine art students unfamiliar with Frederick Jackson Turner or the debates between Asa Gray and Louis Agassiz, for example, her book will be a revelation. Indeed, introducing art historians to other disciplines may be the chief merit of *Nature and Culture*.

Finally, the work points the way toward future studies. The last chapter on European sources of

American art bursts with new possibilities. The notion that artists, scientists, and philosophers were often deeply interested in common subjects provides new areas for investigation. Novak's work on clouds and plants is a prototype.

PETER C. MARZIO
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Washington

BRUCE LAURIE. *Working People of Philadelphia, 1800-1850*. Philadelphia: Temple University Press. 1980. Pp. xiii, 273. \$17.50.

The fashion in recent antebellum labor history has been to posit an "industrial morality," allegedly prescribed by employers, and to characterize workers' culture by whether they succumbed to or resisted it. But the industrial revolution, Bruce Laurie tells us, swept a city like Philadelphia slowly, in uneven waves. It could have required no single "morality." He proposes instead that the city's laborers were grouped into three distinct subcultures, each, moreover, derived from traditions and experiences quite outside the orbit of industrialization.

In the century's early decades what Laurie terms "traditionalists" dominated—lackadaisical workers, unambitious, overly fond of liquor and low amusements, rowdy, and racist. By the 1820s, two resurgent movements with origins in the previous century began attracting followers, especially among artisans. Evangelical preachers promoted moral rigor, self-discipline, and improvement through individual effort and respect for rank. "Rationalist radicals," heirs to Thomas Paine, espoused a "producer ideology," which held the world's wealth the product and rightful property of all who did honest manual labor. Their General Trades Union (1834-39) secured significant gains through strikes.

The Panic of 1837 brought a crucial shift in impetus. The unions fell apart, radicalism faltered. The preachers, calling hard times a retribution for man's depravity, won a flood of new converts. Soon the great Irish migration immensely strengthened the traditionalist group. Their defensive insularity, says Laurie, provoked a powerful nativist reaction. Bloody battles in the streets ensued. Workers became markedly less militant, as traditionalist docility was reinforced by the Catholic clergy's conservative social views, while evangelists hailed the bosses as brothers and clung to the dream of self-advancement. Some "radical revivalists" did manage to rekindle the union movement but with little lasting success. The working class was too deeply riven, too given over to hostile systems of belief, for radicalism then, or long afterwards, to catch its ear.

Unfortunately, Laurie's impressive research has been grafted onto an interpretive scheme he has al-

ready implicitly criticized. His choice of subcultures still presumes workers were defined by their conformity, resistance, or indifference to the new economic order. Yet his own evidence shows that, precisely when that order takes firm root, most workers found their identity in and acted through ethnic, religious, and occupational affiliations. This is why he is at a loss when his subcultures start melding or taking on each others' traits. Furthermore, his collection of radicals is maddeningly amorphous, indiscriminately embracing individuals just because they followed the producer ideology (in fact common among Jacksonians and other capitalists) or joined unions. The distracted, anemic performance of the few real radicals in the bunch makes one wonder about the depth of authentically radical sentiment among Philadelphia laborers. Laurie's "ideal types" ignore or distort large realms of working-class experience. It might well have been better to begin with several broad ranges of attitude, show how workers were distributed across the spectrum, and then determine how the popularity of these attitudes shifted with the currents of industrial and social change.

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FRANCIS X. BLOUIN, JR. *The Boston Region, 1810-1850: A Study of Urbanization*. (Studies in American History and Culture, number 10.) Ann Arbor: UMI Research Press. 1980. Pp. xiii, 220. \$26.95.

This study adds little to scholars' understanding of the process or urbanization in nineteenth-century Massachusetts. Presented as an attempt to test the urbanization models of Allan R. Pred, August Lösch, and several others, Francis X. Blouin, Jr., argues that urbanization occurs within interrelated networks of cities and towns. Cities are not "self-contained economic units," and their relationship to their neighbors is "more complex than a city-hinterland perspective" (p. 135). Considering eastern Massachusetts, 1810-50, few will challenge Blouin's assertions.

Whether Blouin's book constitutes a test of any model, however, is doubtful. Limitations of data, analysis, and prose style give *The Boston Region* a banal quality. Its readers will find themselves nodding, and not only in agreement. The second chapter, which defines the Boston region, seeks to establish a hierarchy of cities and towns based on quantitative measures. The leading conclusion is that Boston was number one. Blouin presents useful information on the location and growth of manufacturing drawn from the McLane report of 1833 and the 1850 census, but his explanations are general and deductive.

Blouin's discussion of transportation is more valuable. Analysis of economic activity and population before and after the advent of railroads shows that trains reinforced existing central places within the 1833-50 period. To scholars familiar with Massachusetts this comes as no surprise, but Blouin deserves credit for establishing the fact.

Chapters on credit and marketing are less satisfactory. Since it is impossible to measure the extent of overall activity, one cannot assess relationships within the region precisely. Relying on impressionistic data, Blouin points to the obvious, that Boston was the key center. The precise boundaries of Boston dominance elude us. Consideration of cases where Boston financiers' reach extended beyond the region and cases where outside creditors penetrated it could have proved illuminating. The elementary character of the discussion is suggested by passages like, "finance can be as simple as well as a complicated process. Essentially finance involves paying for goods and services rendered" (p. 96).

Blouin's leading conclusions will not excite controversy. Boston was certainly "the primary transport node" (p. 133). Indeed it was known as "the hub" long before the invention of systems analysis. Blouin is correct when he concludes that "an expanding economic base permitted increased levels of population. When this expansion was in the specialized manufacturing or service sector which required small amounts of space relative to agriculture, conditions emerged in many of the cities and towns of the region which permitted a considerable population to occupy a small amount of space—a precondition for an urban environment" (p. 134). But such passages lead one to question the general editor of UMI's series in American History and Culture, Robert Berkhofer. One hates to think that Berkhofer believes historians would find such a conclusion enlightening.

RICHARD D. BROWN
University of Connecticut

KENT D. RICHARDS. *Isaac I. Stevens: Young Man in a Hurry*. Provo, Utah: Brigham Young University Press. 1979. Pp. xiv, 484.

Kent D. Richards's thorough study of Isaac Stevens's career examines the major issues involved in the exploration and early settlement of the Pacific Northwest in the mid-nineteenth century. The narrative traces Stevens's early life, education, marriage, and military training briefly. Most of the book, however, concentrates on his actions between 1852 and 1862. Except for graduating first in his class at West Point and for helping Franklin Pierce during the 1852 presidential campaign, Stevens's career differed little from that of most young army

officers. Because he saw little future in the army, Stevens resigned in 1853 to enter politics.

That year he was appointed as the first governor of Washington Territory. At the same time he organized and led one of the four Pacific railroad surveys dispatched by the War Department that year. His party crossed the region between St. Paul, Minnesota, and Seattle in the West. As territorial governor he dealt with the settlers and Indians in a region stretching from the Rocky Mountains to Puget Sound. His dealing with both groups showed him to be inflexible, shortsighted, and controversial. During 1856 Stevens declared martial law in several counties, had judges arrested, turned courts out of session, ran roughshod over the civil rights of the citizens, and forced a series of arbitrary treaties on the regional Indian tribes that led to repeated violence and war.

This study of Stevens's career is indispensable for an understanding of Washington territorial issues. In addition, it demonstrates for historians studying the settlement process in the nineteenth century the ways in which individuals often had more impact than institutions had in some frontier situations. Because of distance, widely scattered population, and slow communications, territorial officials could act almost without restraint if they chose. Stevens's career also indicates how little meaning political labels had during the frontier stage of settlement.

This book is well planned and well written. For readers not familiar with local affairs in the Northwest, some of the detail might seem unneeded, but generally the prose is clear and to the point. Based on a wide range of primary and secondary materials, this book fills the obvious need for a study of Isaac Stevens. The author is even-handed, both praising and damning his subject when necessary. He analyzes Stevens's actions carefully. Although all readers may not agree with his treatment of specific incidents, they can read this book with the assurance that it offers a judicious treatment of an important but controversial figure in the history of the Pacific Northwest.

ROGER L. NICHOLS
University of Arizona

HUBERT H. WUBBEN. *Civil War Iowa and the Copperhead Movement*. Ames: Iowa State University Press. 1980. Pp. xi, 280. \$15.95.

The key to this volume is the word "and" in the title. Primarily a political and economic history with some social history included, it devotes special attention to Iowa's Copperhead experience. Hubert H. Wubben bases the account largely and carefully, but by no means exclusively, on Iowa newspapers. In his quest for Copperhead material, he wisely mined the important Charles Mason papers.

It would be impossible to explain the impotence and frustrations of Iowa's Democrats without considering the Copperhead faction. Even in 1862, when Democrats made important gains elsewhere in the Middle West, Iowa's divided Democrats failed to follow suit. Wubben carefully traces confusion in Democratic ranks over the need some perceived to negotiate with the South and support for the war that others insisted upon. By 1863, the War Democrat majority of the party essentially split with the noisy minority Copperhead faction. Yet overall, the Copperhead movement in Iowa was not an important one, because Republicans held firm political control of Iowa throughout the period. Most Democrats and Republicans agreed, however, in their assessment of blacks. Wubben claims that Iowans regarded blacks as unkindly as they did Indians. Yet in 1865 Iowa Republicans endorsed black suffrage and in 1867 added blacks to the voter lists.

While Wubben takes note of the "Southern Border Brigade," an Iowa militia organization formed to guard against guerrilla incursions from Missouri, he makes no mention of the "Northern Border Brigade," a similar organization formed in 1862 to defend Iowa from the Sioux on the warpath in Minnesota and the Dakotas. Otherwise, he carefully surveys economic conditions in Iowa, and concludes that despite the fears of some, Iowa generally experienced prosperity during the Civil War. He finds little evidence to support any claim of economic conditions influencing political partisanship. A map of Iowa showing the places described in the text would have been useful. Doubtless both Wubben and Iowa State University Press are embarrassed that pages 250 and 251, which contain 60 notes to chapter 7, are blank in the reviewer's copy.

The volume invites comparison on the one hand with other state histories of the Civil War, such as Richard Current's book in the History of Wisconsin series, *The Civil War Era, 1848-1873*, and on the other with Copperhead studies, like Frank L. Clement's *Copperheads in the Middle West*. It is a tribute to Wubben that it ranks favorably in either category.

ROBERT H. JONES
University of Akron

CARL H. MONEYHON. *Republicanism in Reconstruction Texas*. Austin: University of Texas Press. 1980. Pp. xvi, 319. \$19.95.

In this study Carl H. Moneyhon emphasizes that Unionists created the Republican party in Texas as a means of achieving their own political goals. Sam Houston had built a Unionist party, and Provisional Governor Andrew Jackson Hamilton, a

former Texas congressman and Unionist, attempted to reconstruct the state by restoring Unionism as his base of support. One group of Unionists, however, refused to accept Hamilton's concessions to Northerners, identified itself with President Andrew Johnson and identified the Hamilton Unionists with the Radicals in Congress, and with the support of the Democrats easily won political control of the state.

The defeated Unionists, convinced that President Johnson would not exclude the secessionists from power, thereupon persuaded Congress to dispose of the existing Southern state governments and require new constitutions that would mandate black suffrage and disfranchise enough former Confederates to keep them from regaining power. By this action, Moneyhon states, they became Republicans. These same Unionists next held a state convention, in 1867, and there adopted a platform that favored free public schools, endorsed the national Republican platform and the role of the military in Texas, and called for the removal of all officeholders who had supported the Confederacy. After the military commander with whom they allied had removed the unwanted officeholders and the Union League had registered about 98 percent of the adult black males, the Republicans elected most of the delegates to the Reconstruction convention that assembled on June 1, 1868.

In that convention, however, the Republican delegates soon became irreconcilably divided. One group, led by A. J. Hamilton, sided with the agricultural interests, opposed *ab initio* (the doctrine that all official acts of the Texas government since March 2, 1861, were illegal and void), favored limited civil rights for the freedmen, and opposed division of the state. The other group, led by Edmund Jackson Davis and Morgan C. Hamilton, wanted *ab initio*, black suffrage, division of the state, disfranchisement of the former Confederates, and public free schools. When it lost its fight in August to get the Republican party state convention to endorse *ab initio*, the radical group organized a new Republican party with the Union League, then controlled by blacks, as its political base.

Without completing a state constitution, both factions took their fight to Washington, where Congress and the president approved an election in Texas, but at a date to be set later by the president. (The constitution was completed under the direction of the military commander.) In the election in late 1869, Davis won the gubernatorial contest, by a small majority over A. J. Hamilton, but outside his own district he received very few white votes. The Republicans had used black suffrage to attain power, but the radical legislation of the Davis administration was unacceptable to the numerically dominant whites. Consequently, when the federal

government withdrew its support and in 1872 passed the Amnesty Act, they were swept from office and, except for a few leaders, became a party of blacks and federal officeholders.

The research for this study is thorough, the material is well organized and clearly presented, the appendixes are valuable, and the format is attractive. Only six errors (factual, grammatical, and typographical) were noted, and none is significant. Overall, this is an excellent book.

ERNEST WALLACE
Texas Tech University

ELIZABETH JACOWAY. *Yankee Missionaries in the South: The Penn School Experiment*. Baton Rouge: Louisiana State University Press. 1980. Pp. xvi, 301. \$25.00.

The Penn School experiment on St. Helena Island, South Carolina, began shortly after its occupation by Union forces in 1862, when Miss Laura Towne, an idealistic New England abolitionist, arrived to offer her services toward improving the lives of the blacks who lived in that remote coastal region. As a school dedicated to black uplift, the experiment lasted until 1948. A surviving remnant (Penn Community Services, Inc.) exists on the island today. Several schools for the same purpose were funded in other Southern locales by Northern philanthropists during the Civil War and Reconstruction era; Penn School proved to be the best known and the longest survivor.

For forty years, until Miss Towne's death in 1901, she and her Canadian companion Ellen Murray, labored to influence the lives of the black children and adults of St. Helena. Regrettably, little of their accomplishments—or mistakes—is included in Elizabeth Jacoway's otherwise thorough study of Penn School's history. The author notes (p. 31) that the two ladies "kept no records of their school," although "Miss Towne did keep a diary." That diary apparently was of little benefit to Jacoway, for she devotes only five pages to the development of Penn School during the 1862–1901 years. This reviewer is not in a position to declare that other sources exist that would have added at least a bit more light on this formative period of the school, but the author should have indicated—in a footnote or in the preface—whether she had made an effort to know more of Penn School's first half of its history.

For the twentieth century, Jacoway has provided a full and clearly written account of Penn School's successes and failures. On balance, she finds more failure than success, and she does not shirk from saying so in plain language. After Misses Towne and Murray, Penn School was run by Rossa Belle Cooley from 1904 until the 1940s, assisted by another Northern gentlewoman equally imbued with

missionary idealism, Grace Bigelow House. That these were dedicated, unselfish people the author recognizes. Jacoway also concedes that for those Sea Island blacks who came under its influence and control, Penn School improved living standards and broadened conceptions of life. But Jacoway emphasizes what she perceives as fundamental flaws in the Penn School experiment, and she makes a strong case. Outsiders were trying to tell island black people how they should live. Although well intentioned, these outsiders were endeavoring to smother black culture: to transform blacks into white people in all but pigment of skin. Misses Cooley and House, along with the Northern trustees who supported them, hoped to solve the "Negro problem" by demonstrating that rural Southern blacks could be changed into people who were culturally no different from the majority of Americans. A baser motive for supporting Penn School, Jacoway suggests, is found in several statements by its trustees, and in the Annual Report of 1909, which stated, in effect, that blacks should be taught how to become satisfied with a life of farming in the rural South, so they would not create problems by flocking to Northern cities. Jacoway sums up Penn School by saying that "it all grew out of northern needs and assumptions, and from the first it sought to dispel northern fears."

This is a damning indictment of the Penn School experiment, but Jacoway has much evidence to support this somber conclusion.

WILLIAM I. HAIR
Georgia College

RICHARD H. BRADFORD. *The Virginus Affair*. Boulder: Colorado Associated University Press. 1980. Pp. xvii, 180. \$10.00.

In October 1873, the steamship *Virginus* was seized by a Spanish gunboat on the high seas near Cuba, which was then wracked by the ten-year insurrection against Spain. The *Virginus* was a notorious *insurrecto* gunrunner and was carrying a military expedition when captured. It was listed under American registry, which later proved to be fraudulent, and flew the American flag, while the ship's officers and many of the expedition members claimed United States citizenship. Taken into Santiago de Cuba, fifty-three of those aboard were promptly shot as pirates, before a British warship arrived to protect the British subjects among the captives. This stopped the slaughter, already sufficient to start an international incident and set off a wave of furious indignation in America.

The resulting crisis peaked in November, when

an ultimatum by Secretary of State Hamilton Fish gave the Spanish government twelve days in which to accede to his demands for apology and reparation. The sobered Spaniards began to come around, while American claims were tempered by the realization that their legal case was at best shaky. The *Virginus*, it was soon shown, was owned and employed by the Cuban revolutionary junta; only a small minority of those aboard it appeared to be bona fide American citizens, and it had no legal right to fly the American flag. The Spanish authorities, on the other hand, had shot dozens of men without proper legal process and ultimately paid indemnity for their act to both the British and American governments. The entire matter was peaceably settled by the end of the year and passed into the pages of treatises on international law as a classic case study.

Richard H. Bradford retells this story in 139 pages of text in *The Virginus Affair*. Well written and readable, his account suggests modest revisions of the standard textbook version. The British role in resolving the conflict has previously been noticed, but Bradford clearly shows the influence of the British government and its ambassador in Madrid in persuading the Spanish to meet American terms. Furthermore, London had its own claims in the matter on behalf of the British citizens involved, and the sane and moderate handling of the Anglo-Saxon portion of the dispute proved a useful guide for American diplomacy. Earlier it was the British navy that prevented the execution of the remaining ninety-one *Virginus* captives, which would have made a peaceful solution harder to achieve. The episode was in fact an American-British-Spanish diplomatic triangle, and Bradford explains it as such.

It was also significant that the Spanish government with which the United States dealt in the matter was a short-lived republic, headed by one of Spain's leading liberal democrats. The ideological sympathy thus aroused in the United States helped to offset the initial public anger, and some American voices warned that a successful war with Spain would destroy the fragile republic and bring back a restored monarchy (which in fact occurred anyway just after the general terms of settlement had been reached). In this and other respects the author keeps us aware of relevant European factors in his story.

Less satisfactory is the book's concluding chapter, which attempts comparisons between the peaceful resolution of Cuban-Spanish tensions in 1873 and their degeneration into war in 1898. Curiously, Bradford virtually ignores the intellectual revolution of the 1890s that led many Americans to accept imperialist premises and rethink the international posture of the United States, as well as the chauvinistic jingoism that burgeoned at the time. This

failure to consider their changing context dooms the comparison of mere events to sterility.

DAVID HEALY
University of Wisconsin,
Milwaukee

RONALD STORY. *The Forging of an Aristocracy: Harvard and the Boston Upper Class, 1800-1870*. Middletown, Conn.: Wesleyan University Press; distributed by Columbia University Press, New York. 1980. Pp. xv, 256. \$16.00.

Harvard College was founded and governed for almost two centuries by the state-church establishment in Massachusetts. This book is a thorough and extremely interesting analysis of how, between 1800 and 1870, it gradually became a privately endowed institution supported and governed by upper-class Boston. In 1780, when the Massachusetts constitution was written, for instance, the Board of Overseers was composed of the president of the college and the governor, lieutenant governor, councilors, and senators of the state, as well as the Congregational ministers of Cambridge, Watertown, Charlestown, Boston, Roxbury, and Dorchester. A hundred years later, these magistrates and ministers had been replaced by Harvard alumni drawn primarily from the ranks of Boston's upper-class business and professional community.

Harvard continued to prosper under state control in the nineteenth century so long as the Federalists, the party of the rich and well-born in Boston and elsewhere in the state, dominated all branches of the state government. The real break came in 1824 when the Federalist party was finally defeated forever in Massachusetts; the triumphant Jeffersonian-Republicans cut off all state funds.

The transformation of Harvard from a communal to a class institution was no easy task: it was led by a small group of increasingly affluent, upper-class Bostonians who, in 1842, founded the Harvard Alumni Association and in 1855, the "Club of the Graduates of Harvard College," which later became the Boston Harvard Club; even then it was not until 1865 that the Massachusetts legislature finally transferred control of the college to the Harvard alumni who were henceforth to choose the Overseers "voting at Cambridge on Commencement Day." The margin of victory was a single vote in the senate and a "slender majority" in the house.

Ronald Story shows how the privatization of Harvard turned it into an elite institution in which the socializing function became as important as teaching and research. And the goal of the socializing process was the cultivation of gentility in the style of Oxford and Cambridge in England. Under

the leadership of Edward Channing, professor of rhetoric after 1819, the so-called Harvard accent was modeled on "the purity and simplicity of the English style."

Story carefully shows how the privatization of Harvard was part of a more inclusive development of an associational rather than a solely familialist upper-class in Boston; at the same time, as Boston's brahmins were building Harvard into the most highly endowed private institution in America, they were also building up a solid group of other cultural and medical institutions such as the Lowell Institute, the Athenaeum, the Massachusetts Historical Society, the Massachusetts General Hospital (McLean Asylum), the Perkins Institute for the Blind, and so forth. All of these institutions were founded before the Civil War and formed the core of what Tocqueville saw as our American associational democracy, a democracy unlike anything found on the continent of Europe where all education and most cultural institutions have traditionally been founded and supported by the state.

As there was very little to criticize in this excellent book, it was all the more boring to have the author close on a now-fashionable anti-elitist note: "For the forging of the Brahmin aristocracy, then," he writes in the last paragraph of his book, "the consequences of the great institutional developments of the antebellum era were altogether happy. For the forging of a viable society with a coherent culture, they were far less so." He, of course, produces no evidence for this final concession to current tribal sentiment.

E. DIGBY BALTZELL
University of Pennsylvania

R. A. BURCHELL. *The San Francisco Irish, 1848-1880*. Berkeley and Los Angeles: University of California Press. 1980. Pp. viii, 227. \$16.95.

This is a detailed and well-documented account of the Irish arrival and settlement in San Francisco. Their presence predated the city's existence. R. A. Burchell, of the University of Manchester, continues their story through the history of an expanding metropolis to the demise of the Irish-led Workingmen's Party of California.

This vigorous analysis is most significant because it validates and expands, through quantitative methods, the interpretation that nonquantitative historians have advanced against the standard views on American immigration history. These western Irish were neither uprooted nor slum dwellers.

The nineteenth-century Irish who reached the new and booming San Francisco had previous ur-

ban experience, were upwardly mobile, and enjoyed family stability and comprehensive support through their own elaborate networks of religious, economic, political, and social institutions. They had no single profile but seemed to be everywhere, doing everything. As a stable, organized, and large minority within a diverse, chaotic, instant city, the Irish found what they wanted in America—material success and personal and group satisfaction. With these went cultural pluralism but not structural assimilation.

Irish success in politics, for example, was immediate and spectacular. Burchell's chapter "The Politics of Adjustment" is the most thorough treatment available of this variation from the accepted Irish-American political norm.

The study rests upon rich source materials: raw political, economic, social, and environmental data as well as a thorough examination of the contemporary press and obscure but valuable articles and monographs. The volume is not without flaws, however.

The remains of Terence B. McManus, a former Irish rebel whose 1861 funeral set in motion the Fenian movement (which in the U.S. culminated in the 1866 "invasion" of Canada by Irish-American veterans of the Union army), did not travel across country. There was no transcontinental railroad until 1869. His body went by way of Panama to New York and then on to Ireland. Of greater concern should be the half-correct statement that the Irish in San Francisco felt threatened by the Chinese and by blacks.

Blacks were not sufficient in number to threaten anyone during this period. Existing literature on the California Chinese has established the antagonistic relationship between Asian and Irish laborers. There is no evidence, however, in this volume or elsewhere, to support the incorrect assertion that a similar relationship existed between Irish and blacks in San Francisco.

This is a welcome addition to the limited scholarship on San Francisco as well as a valuable refinement in the history of American immigration.

JAMES P. WALSH
University College,
Cork

MARY A. HILL, *Charlotte Perkins Gilman: The Making of a Radical Feminist, 1860-1896*. (American Civilization Series.) Philadelphia: Temple University Press. 1980. Pp. xi, 362. \$14.95.

Given the appeal of biography, it is surprising that Mary A. Hill's work is the first full-scale analysis of the life of Charlotte Perkins Gilman, the pre-emi-

nent feminist ideologue of the early twentieth century. Yet until 1966, when Carl Degler edited a new edition of Gilman's *Women and Economics* (1898), her name had been lost to the historical record, another casualty of the tendency of Americans to forget that they had a feminist past.

Hill is to be congratulated not only for providing an analysis of Gilman's life but also for writing a lively and convincing book. Hill moves with assurance from Gilman's personal life to her public life and, deftly linking the two, meets the hardest challenge of the art of biography. Hill's accounts of the development of Gilman's feminist consciousness and of her marriage to Walter Stetson and her consequent nervous breakdown (the best-known incident of her career) put in order a puzzling episode. Hill's argument that, at least during the early California years, Gilman's love relationships indicated a commitment to sexual freedom is interesting, although Hill does not sufficiently clarify Gilman's beliefs on the matter, given the usual assumption that she supported the contemporary feminists' belief in the "single standard of morality" and their aversion to free indulgence in sex. Hill's narrative also sheds light on the relationship between radicals and academics in this period and on progressive strains within the home economics movement, although one wishes she had indicated more fully how these relationships influenced Gilman's mature thought.

Yet the book's greatest strength is also its greatest weakness. Hill's identification with Gilman contributes to a sensitive understanding of her subject, but it also leads her to include lengthy block quotes from Gilman's writings that obfuscate rather than enlighten. Moreover, long passages lingering over minute changes in Gilman's emotional life reveal little more than the normal vagaries in the emotional life of any individual. There is no reason for the completed biography to be two volumes in length, especially since the biography is not really definitive: an oblique footnote on page 126 indicates that Hill has not seen all the family papers.

Charlotte Perkins Gilman is a subject rich for analysis, and the sizable collection of her extant papers should keep scholars busy for decades. Yet despite the attention that book-length studies invariably receive, perhaps the most insightful analysis of Gilman's life may still prove to be Carol Berkin's brief "Private Woman, Public Woman: The Contradictions of Charlotte Perkins Gilman," contained in *Women of America: A Brief History*, an article anthology that Berkin and Mary Beth Norton recently compiled.

LOIS W. BANNER
University of Maryland,
Baltimore County

W. A. SWANBERG. *Whitney Father, Whitney Heiress*. New York: Charles Scribner's Sons. 1980. Pp. xiii, 518. \$17.50.

This ambitious book by one of America's foremost popular biographers is really three biographies in one. The first book is a biographical treatment of William C. Whitney, extending from his marriage in 1869 to his death in 1904. The second book combines a biographical account of Whitney's youngest daughter, Dorothy, through her thirty-third birthday—she lived to be 80—with an even fuller account of the husband of her brief first marriage, Willard Straight.

In compiling this work, W. A. Swanberg was fortunate enough to have access to candid diaries and correspondence in the Dorothy Whitney Straight Elmhurst papers at Cornell University. Although the author strives to be unbiased, most of the book's principals fare poorly under his treatment. Only Dorothy comes out unscathed.

William C. Whitney, the charming, ambitious young man of modest means, after marrying a somewhat shrewish, party-giving heiress, went on to help his government pioneer the New Navy in the 1880s and then threw away his talents in a scramble for personal wealth. The author contends that Whitney, unlike those robber barons who "lightened their sins by a measure of achievement," left behind nothing positive.

Still, the admittedly gifted Whitney *did* leave Dorothy. Unlike most girls not faced in childhood by sibling rivalry—her sister and brothers were much older than she—Dorothy seems to have grown up with none of the problems of an only child. This was especially startling since her father gave her next to no personal attention. Moreover, she grew up to be the only really socially conscious member of the family.

The equally gifted Willard Straight rose in upstate New York from near poverty to marry Dorothy and then squandered his abilities leaping from one illusory goal to another until he died suddenly of influenza during the epidemic of 1918. Had Straight, thinks Swanberg, been less ardent an imperialist and a frustrated Wall Street financier, he might have lived to become a successful artist, writer, and editor. The unspoken message of the book is that history abounds in people perpetually misusing their talents.

Swanberg admirers will find this volume up to the generally high standard the author established in 1956 when he placed before the world of scholarship his *Sickles the Incredible*. But the book suffers from occasional lapses into speculation. For instance, Swanberg extrapolates from Whitney's pall bearers the identities of his regular dining companions: "If it can be assumed that one's pallbearers are

picked from favorite dining companions, he ate with Ryan, Widener, William Jay, J. P. Morgan, Hamilton McK. Twombly, and a few others including Elihu Root when Root was in town" (p. 169).

WILLIAM M. ARMSTRONG
Potsdam, New York

SALME HARJU STEINBERG. *Reformer in the Marketplace: Edward W. Bok and The Ladies' Home Journal*. Baton Rouge: Louisiana State University Press. 1979. Pp. xix, 193. \$12.95.

In 1889 Cyrus Curtis, a publisher bent on commercial success, entrusted his new *Ladies' Home Journal* to the editorial autonomy of Edward Bok, a writer of reformist convictions. Curtis's judgment was astute: in the next thirty years Bok's mild crusades made the magazine an immensely profitable enterprise. Struck by the improbability of so successful a marriage of "commerce and good works" (p. 147), Salme Harju Steinberg attempts to explain why. Her account rests on an extensive study of the *Journal's* contents as well as published memoirs and unpublished materials such as Bok's correspondence, archives of the *Journal* and the Curtis Company, and some wartime records of the Wilson administration.

From the first, Bok, with experience as the advertising manager of *Scribner's*, understood that a successful magazine had to offer advertisers a commercially attractive audience. To do this he insisted—to a degree unusual in his day—on the editorial wisdom of rejecting requests for favors from advertisers and from the business office in order to build a body of subscribers whose loyalty rested on their perception that the magazine and its editor could be trusted to remain independent from direct commercial influence. Consequently, to a generation of women learning to raise families and manage households in a rapidly industrializing society the *Journal* offered three mutually reinforcing features of irresistible interest: entertainment that would stimulate but not offend urban middle-class sensibilities; advertisements screened in part for their reliability; and advice for coping with unfamiliar social problems, advice that was always practical, usually "predictable and reassuring" (p. xvi), and sometimes inspirational in its defense of progressive ideals. Bok preached that self-discipline and hard work won happiness, that character, more than circumstance, determined fortune, and that America's social problems would yield to the deliberate efforts at reform on the part of men—and women—of good will. At first Bok advised women to remain in the home and eschew the vote. By 1914 he was devoting a substantial part of his magazine to urging women to train for careers and

to play a major part in civic reform, temperance (the *Journal* only reluctantly favored prohibition), better food and drugs, improvement of family and public health, a clean-up of the physical environment, and the drastic liberalization of the public school curriculum, particularly to provide meaningful education for girls. During the First World War Bok supported Wilson's economic mobilization and military strategy but avoided patriotic fervor and encouraged his readers to keep alive at home the humane goals of the reform movement. By 1919 at his retirement (which Curtis welcomed by then) he was becoming more, not less, of a liberal idealist.

In the increasing commercialization of the popular press in the twentieth century, how long could a reform journalist last? About five years, concluded Regier and others a generation ago. Their analysis now seems superficial. Bok lasted thirty, and in Steinberg's effort to account for his success she offers the history of mass media a distinct service by concentrating much more fully on the beginning than on the denouement. Indeed, the opening chapter, the most useful in the book, provides a detailed and probing analysis of the complex considerations that entered into the establishment of the modern woman's magazine. The study as a whole generates two frustrations, however. One is a persistent awkwardness in wording and phrasing that frequently confuses the meaning. (The author surely means, for example, "buying habits" instead of "purchasing power" on page 2. There are at least a dozen more. It is regrettable that a good editor did not come to the scholar's rescue in these cases.)

A second frustration arises in the concluding chapter, where an examination of the significance of Bok's virtually forced resignation seems strangely evasive and incomplete. The author salutes the "prophetic witness" of an editor whose broadening perspective on "race, sex, and class cost him his job" (p. 149). But her evidence is inconclusive. All Curtis required was that Bok "modernize" (p. 143) the magazine so as to expand its audience, and by 1919 the editor was unwilling to do this. One supposes that Curtis objected not to Bok's liberalism or to his concept of editorial independence but rather to the prospect that after thirty years his editor had no more fresh ideas about how to gain more readers. Therein lies, perhaps, the unremarkable, even commonplace, explanation that Steinberg so assiduously seeks for the successful partnership between a publisher bent on profits from advertising and an editor bent on good works. Editors were free to print whatever they pleased so long as they held the interest of a large number of people. That was inherently the reward and the risk of the new commercial media.

OTIS A. PEASE
University of Washington

WILLIAM DUSINBERRE. *Henry Adams: The Myth of Failure*. Charlottesville: University Press of Virginia. 1980. Pp. 250. \$20.00.

This monograph concentrates on the period between 1872 and 1892 that Henry Adams refused to write about in his autobiography. William Dusi-berre argues that Adams wished to hide what he thought to be his real failures during this period—the suicide of his wife and the lack of acclaim for his nine-volume *History* of the Jefferson and Madison administrations. Dusi-berre contends that Adams was not to blame for his personal tragedy and resurrects the *History* from the comparative lack of recognition it has achieved.

Adams and his wife, Clover Hooper, writes Dusi-berre, had a warm and close marriage, marred only by her periodic depressions. More important, she helped and inspired him in writing the *History*. Dusi-berre's treatment of the marriage—and indeed of several other relationships Adams had—is a backdrop to his defense of this work, "the most valuable contribution of any American to the writing of history" (p. 161). Although a full understanding of Dusi-berre's discussion requires a more detailed knowledge of the politics of the early national period than he provides, his defense and explication of Adams's work occupy almost one hundred pages of his book, well over 40 percent of his text. Another chapter of some thirty pages urges that *Mont-Saint-Michel and Chartres* and the *Education* are inferior to the *History*.

Dusi-berre defends the greatness of the *History* as art and as science. By scientific history, he means history that has persuasive interpretations sustained by the primary evidence. He finds Adams's scholarship "a Gothic cathedral" that "stands in need of substantial restoration" (pp. 159, 161). While this is a modest judgment, Dusi-berre is enthusiastic in his defense but is, it seems to me, more successful in establishing the *History* as a literary masterpiece. Here, however, he assumes that the canons of literary judgment are transtemporal and neglects to confront the possibility that nine volumes of prose in the style of Gibbon might be a surfeit of goodness to a contemporary reader.

Dusi-berre's argument is weakened because he does not self-consciously consider the grounds that have enabled historical works to endure. Appraisals are surely based on literary merit and ability to withstand later scholarly attack. But these are not the only reasons that, say, Herodotus's *Histories*, Gibbon's *Decline and Fall*, and Burkhardt's *Civilization of the Renaissance* have become classics. We think of these works as telling us not only about the period they study but also about something deeply significant in the intellectual culture that produced

them. Dusinberre does not explore this issue with Adams's *History* in mind.

BRUCE KUKLICK
University of Pennsylvania

DAVID FREEMAN HAWKE. *John D.: The Founding Father of the Rockefellers*. New York: Harper and Row. 1980. Pp. vi, 260. \$12.95.

This is a marvelously written book. It is a concisely developed, personal, and business biography of the Rockefeller founder. The story begins with John D. Rockefeller's "scoundrel" father, then traces the younger Rockefeller's early upbringing and training in business. This is followed with a balanced and informative account of the rise of Standard Oil Company and his role within it. These chapters are interspersed with assessments of Rockefeller's personality and his relations with family, friends, business allies, and enemies. The book closes, somewhat quixotically, not with his death in 1937, but instead with his being awarded a service button by Standard Oil in 1922.

The strength of the book lies in David Freeman Hawke's ability to detail clearly the workings of American industry, especially the oil industry, in the late nineteenth century. Yet this clarity should not be mistaken for oversimplification. He addresses issues of comparative business efficiency, market strategies, and financing in a manner that conveys their complexity without sacrificing clarity. The book's other major strength lies in its ability to evoke a sense of the humanity of its subject. He is not uncritical of Rockefeller, but that is balanced with a sensitivity to him as a human being with feelings, morals, and emotions: not simply an adding machine greedily counting up the day's profits. Hawke's intention is to provide a full human picture, not a caricature.

Having said all this, however, one comes to the central question about the book: for whom is it intended? Neither the author nor publisher gives a clear indication of this. Nonetheless, they leave an impression that something new or different in terms of research or conclusions about the elder Rockefeller is being presented. This is simply not the case. However felicitously the book is written, it is merely a restating of facts and ideas long known to anyone with more than a nodding acquaintance with the voluminous Rockefeller literature. I can only assume, then, that the book is intended as a convenient and short (229 text pages) introduction to Rockefeller and business in the late nineteenth century for the general public and undergraduate students. For the former group, I think the book succeeds admirably. For the latter group, it is certainly adequate, although I still prefer Glenn Por-

ter's *The Rise of Big Business, 1860-1910*, which is of broader gauge, more insightful in its analysis, and equally well written.

JOHN N. INGHAM
University of Toronto

THOMAS A. BRYSON. *Tars, Turks, and Tankers: The Role of the United States Navy in the Middle East, 1800-1979*. Metuchen, N.J.: Scarecrow. 1980. Pp. xiii, 269. \$14.00.

Diplomatic historians of American foreign policy in the Middle East will find this work to be a valuable reference covering U.S. naval operations in the region, which Thomas A. Bryson defines as "the layer of Muslim states that lie on the southern littoral of the Mediterranean [that is, North Africa], the waters of the Red Sea, the Persian Gulf, and . . . Turkey" (p. ix). A survey based largely on secondary sources, it outlines naval operations and gives names of ships, forces, and commanders that often go unnoticed by non-naval historians of modern Middle Eastern history. Unfortunately, the author stuffs his narrative with the superfluous "U.S.S." prefix for many vessels, as well as with numbers of guns, names of captains, ship tonnages, and hull numbers—the very kind of navalese jargon that helps to deter nonspecialists.

Ignoring the contrived and imprecise title, the reader learns quickly of the continuity of American naval activity vis-à-vis the Arab world around the Mediterranean region since the first Barbary war, a presence that was only sporadic until the creation of the Sixth Task Fleet in 1948. More than half the book deals with the years since then; for the first time in one volume we have the naval details of the Suez and Lebanon crises of 1956-58 and 1970-75. By contrast, the detailed accounts of the Barbary wars of 1801-07, the Algeiras crisis of 1904-05, and the North African landings of 1942 are fairly familiar events to diplomatic and naval historians. Fuller treatments, however, should have been given to Admiral Mark L. Bristol's yeoman work in stabilizing the Middle East after World War I and to the debate with the British navy over the establishment of supreme U.S. naval command in the Mediterranean in 1950-51.

Bryson gives a good, concise explanation of the specific composition and functions of the Sixth Fleet, outlines Russian naval activity in the region since 1954, and concludes with a review of current tactical problems facing the navy in carrying out U.S. Middle Eastern policy. The photographs are mostly of ships and not of events (which circumscribes their utility, since a ship is simply a ship to the nonspecialist), and several cute anecdotes seem

out of place in this otherwise straightforward monograph. The notes and bibliography are very useful.

CLARK G. REYNOLDS

Charleston, South Carolina

BENJAMIN FRANKLIN COOLING. *Gray Steel and Blue Water Navy: The Formative Years of America's Military-Industrial Complex, 1881-1917*. Hamden, Conn.: Shoe String Press. 1979. Pp. 286. \$19.50.

In the 1880s, when the United States Navy, the first of the services to modernize, moved into all-steel construction, successive administrations faced the question of whether the armor and ordnance needs of the "New Navy" would be met by foreign manufacturers, domestic industry, or government-controlled plants. The American steel industry, which initially lacked interest in and capacity for armament manufacture, soon saw its opportunities. Andrew Carnegie might lecture the government on the evils of building weapons for destructive purposes, but he and his partners were also quick to realize that there were "millions for us in armor." Wilson's secretary of the navy, Josephus Daniels, was by no means the first cabinet member to complain that he was "at the mercy of the three manufacturers of armor plate whose policy it is to make the government pay prices far beyond fair profit" (p. 199).

Gray Steel and Blue Water Navy is a first-step exploration of the formative years of America's first military-industrial complex, an analysis of the intersection of naval, business, and technological history from the 1880s to World War I. It chronicles the process whereby the three giants—Carnegie, Bethlehem, and Midvale—came to dominate the manufacture of naval armor and the various (and usually less than effective) ways that successive secretaries of the navy sought to cope with the increasingly complex, politically sensitive issues spawned by naval contracting and changing technology.

In the Populist era "Pitchfork Ben" Tillman led the onslaught against the high cost of armor and the profits of the "robber barons." But after 1900 these political battles stilled. The steelmen, abandoning cut-throat competition with each other, collaborated in submitting virtually identical bids, maintaining stable prices, and agreeing to share lucrative government contracts, while Roosevelt's and Taft's secretaries of the navy, anxious to increase American naval strength, acquiesced to "industry demands [that] meshed with the stability and cooperation which the Republican administration and Republican corporations wanted desperately" (p. 217). This "special relationship" between the trusts and government was, however, anathema to the Wilson administration, which finally succeeded in 1916 in enacting long-debated legislation to estab-

lish a government-owned armor plant that would, it was hoped, serve as a "yardstick" for fair prices.

Yet despite sporadic attempts to control prices and restore competition, most secretaries became "militarized," were guided primarily by their desire to secure the needed armor and ordnance, and ended up by sponsoring a system that "married" business and government. "What they wrought," Benjamin Franklin Cooling writes, "was all of the circumvention of rules and regulations, accommodated principles, and power plays which became main threads of the later military-industrial complex" (p. 216). Significantly, the government-owned "yardstick" never produced appreciable amounts of armor; it fell into disuse in the 1920s; and in the New Deal era Daniels's onetime assistant, Franklin Roosevelt, showed no interest in reviving it.

Cooling's analysis of these events is not always successful. His prose style mutes controversies, often complicates rather than clarifies. There is irrelevant material on routine naval administration. The effectiveness of *Gray Steel and Blue Water Navy* is lessened by a strictly chronological organization that focuses on the work of numerous, often faceless, secretaries of the navy (Roosevelt alone had six, most of whom were being groomed for other assignments). In consequence, the book only fully explores the secretarial dimension of a complex triangular relationship involving the executive branch, Congress, and private industry. Cooling is well aware of the charges of collusion between government and industry and of the allegations of unfair profits and practices, but his attempt to be even-handed and dispassionate frequently leads to an on-the-one-hand, on-the-other interpretation, which suggests that the "marriage" was inevitable, a simple trade-off in which both industry and the navy profited. (For example, "he [Secretary Herbert] . . . was pragmatic enough to realize that Populist radicalism on armor prices and overly ardent prosecution for fraud might endanger a useful, even necessary relationship" [p. 137].) The book is also marred by many jarring typographical errors, the most serious of which is that page 53 repeats *in toto* the contents of page 42, a flaw that destroys the coherence of an entire chapter. *Gray Steel and Blue Water Navy*, in short, opens up an important subject but does not provide the definitive analysis.

RICHARD D. CHALLENGER
Princeton University

THOMAS G. DYER. *Theodore Roosevelt and the Idea of Race*. Baton Rouge: Louisiana State University Press. 1980. Pp. xiii, 182. \$14.95.

A revision of a doctoral thesis, this is a modest but significant book. It is modest in sustaining the strict

boundaries of a thesis. It is an intensive study of Roosevelt's published and unpublished writings to illuminate the place that the idea of race held in Roosevelt's understanding of American historical experience and the relationship of that experience to the entire sweep of human history. It is significant because it presents a well-documented argument that the idea of race always was central to Roosevelt. It informed the way in which he lived his personal life, his historical writing and those of his writings that Roosevelt believed were scientific, his letters, and his role as a politician. Thomas G. Dyer rightly sees his book as revisionist. No previous Roosevelt scholar has postulated the centrality of the idea of race in Roosevelt's cultural imagination.

Dyer proceeds systematically from his opening chapter, "The Racial Education of Theodore Roosevelt." Here he points out how pervasive a sense of Anglo-Saxon racial superiority was within both Roosevelt's informal and formal educational experiences beginning in his earliest childhood. In subsequent chapters on Roosevelt's specific understanding of the relationship of that Anglo-Saxon racial superiority to American Indians and to American blacks, Dyer finds Roosevelt celebrating the Anglo-Saxons' conquest of the Indians with whom they refused to intermarry and whom they were able to replace as the dominant population on the landscape. Blacks were much more troubling to Roosevelt because unlike Indians they had a higher birthrate than Anglo-Saxons and were not a vanishing race. Dyer is sensitive to the changes in Roosevelt's understanding of race including his shift from the term Anglo-Saxon to the term English-speaking race. This shift related to Roosevelt's loyalty to Lamarckianism, which enabled him to believe that the biologically inferior immigrants from Southern and Eastern Europe could acquire characteristics from the superior Anglo-Saxons. These environmentally determined characteristics would then become part of the heredity of the children of the immigrants. Increasingly obsessed by thoughts of race suicide because of the declining birthrate of Anglo-Americans, Roosevelt was willing to accept the children of the immigrants as a defense of whites against the superior breeding power of the colored races. Roosevelt used the term caste, a term rediscovered by historians in the 1960s. Dyer's book invites the question of the assimilation of the new immigrants to the caste outlook of the Anglo-Americans, which insisted on the immutable separation of the white and black worlds.

DAVID W. NOBLE
*University of Minnesota,
Twin Cities*

WILLIAM C. WIDENOR. *Henry Cabot Lodge and the Search for an American Foreign Policy*. Berkeley and Los Ange-

les: University of California Press. 1980. Pp. xi, 389. \$18.50.

William C. Widenor proves that it is possible to write an interesting and believable book about Henry Cabot Lodge. He even manages to introduce an element of sympathy for that quintessential stuffed shirt, the petrified dandy of Nahant, Massachusetts, in his well-written intellectual biography. Widenor studiously limits himself to Lodge's ideas about foreign policy, eschewing all discussion of personality. He unearths "a fairly consistent set of beliefs about the way the foreign policy of the United States ought to be conducted." However indefensible certain of his theories, Lodge concentrated on some "enduring problems," among them "the conduct of foreign policy in a democracy, and the relationship between force and peace" (p. ix).

We learn that Lodge's studies of his Federalist forebears led to a considerable distrust of full-blown democracy. He embraced a "Rooseveltian solution" that emphasized "practical idealism." "The American national state was man's last and best hope on earth, and if it was to fulfill its mission the state itself had to be made strong and secure" (p. 163). These influences shaped Lodge's behavior well before his epochal contest with Woodrow Wilson. In an excellent chapter entitled "The Heir of Jefferson and Buchanan," Widenor traces the development of Lodge's antipathy toward Wilson. This information serves as a prelude to a long account of the struggle over the League of Nations, one that improves on previous such expositions.

Somehow Widenor escapes the problems that plague many authors of first books. He is not afraid to register the expectable and the obvious, should the evidence lead him in those directions. He does not strain for the new and the startling, should the evidence suggest reinforcement of the established view. He manages to avoid both preposterous muckraking and adulatory canonizing. The outcome is a fresh and credible interpretation of interest to scholars and practitioners interested in the future as well as the past.

To be sure, more remains to be discovered about Lodge. Widenor's restricted concentration on ideas precludes much penetration into the psyche of a thoroughly emotional politician. Why did he love T.R. so much and Wilson so little? What role did women play in his life? How did he feel about the immigrant population of the state and the nation he served for so long? Such questions remain for others, who will benefit greatly from Widenor's analysis.

Lodge's pessimism apparently had much to do with his ultimate decline as a historical actor. What if he had drawn upon the Lincolnian tradition rather than the Federalist persuasion? He might then have recognized the possibilities as well as the

pitfalls of life in America the Beautiful. Widenor concludes with a comparison of Walter Lippmann and Lodge. The Senator "wanted the United States to become a responsible world power and often in his later years despaired of its doing so" (p. 350). Like Lippmann, he wondered whether a democratic nation could hope to conduct sound foreign policy. These doubts vastly influenced his behavior, and they limited his achievement.

DAVID F. TRASK
Department of State

S. D. LOVELL. *The Presidential Election of 1916*. Carbondale: Southern Illinois University Press. 1980. Pp. viii, 229. \$22.50.

This study of the 1916 presidential contest is traditional narrative political history. One would like to add "at its best," but, regrettably, such is not the case.

S. D. Lovell has done extensive research in manuscript collections—although, except for the House papers, only those in the Library of Congress—and in newspapers. His most significant contribution in the way of new information is on the details of the parties' organizational efforts and campaign strategies. He is perceptive regarding the dilemmas faced by Hughes in challenging Wilson. The GOP standard bearer could find little—other than the Adamson Act—to criticize in the administration's domestic record, given his own political views, the importance of not alienating former Progressive party voters, and the country's prosperity. Even more difficult was how to attack Wilson for his lack of forcefulness in foreign affairs without appearing a warmonger. And although Lovell does not make the point explicit, his evidence shows how the Democrats capitalized on the situation by what was a demagogic exploitation of the peace and hyphenism issues.

On the other hand, much of the book retreads familiar ground, such as the preconvention maneuvering by Republican and Progressive leaders and Hughes's California troubles. Lovell's overall conclusion reaffirms the long-established view that Wilson won because of the peace and progressivism issues in "the West" (p. 182). Although he does make an attempt to analyze voting behavior, his highly simplistic methodology falls short of the sophisticated quantitative techniques required by present-day standards to substantiate his findings regarding Wilson's strength among farmers and urban laborers. The limitations of his approach are graphically demonstrated by his explanation of the results in Ohio, which, more than California, was the pivotal state in the final result. "In that state," he writes, "the Democrats managed to have every factor to favor them. Most of the voters desired peace. Farmers

were receiving big prices for their crops. Laborers were employed at relatively high wages" (p. 177). But why did these factors not produce Democratic majorities in neighboring Pennsylvania or in any of Ohio's fellow east-north-central states? Thus, he is reduced to blaming Republican "Factionalism"—which was not restricted to Ohio and which appears hardly sufficient to explain Wilson's 90,000-vote margin in what was traditionally a GOP stronghold.

What is most disappointing about this book is the sloppiness of the writing. There are too many dubious generalizations (for example, "the progressive movement was a lineal descendant of the populist, agrarian revolt" [p. 1]). There are too many misleading or simply incorrect factual statements; even the quotations are not always accurate. And what can one say about the appearance in a work published by a reputable university press of such stylistic horrors: "The nominating speeches were begun by New York. . . . Demonstrating her split . . . she placed in nomination" (p. 44); "The question of the extent of loyalty of certain groups of Americans comparatively recently transplanted to this country placed a great strain upon the basic homogeneity of this great melting pot" (p. 60); or "the papers began the use of news, pictures, and cartoons propagandistic in total effect toward creating a bad impression of and questioning the loyalty of a large group of American citizens" (p. 62).

JOHN BRAEMAN
University of Nebraska,
Lincoln

EVANS C. JOHNSON. *Oscar W. Underwood: A Political Biography*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1980. Pp. xvi, 480. \$27.50.

If, among the political leaders who emerged from the New South at the beginning of the twentieth century, Woodrow Wilson ranked at the top and a demagogue like James K. Vardaman at the bottom, Oscar W. Underwood of Alabama (1862–1929) stood squarely in the middle. Born to a prominent Kentucky family, Underwood was raised in Louisville and St. Paul, Minnesota, and studied law at the University of Virginia before settling in Birmingham in 1884. His political fortunes rose with the booming industrial city; he was elected to the House of Representatives in 1894, though later unseated, and re-elected continuously from 1896 to 1914, when he successfully ran for the Senate, where he served until 1927. Underwood became assistant leader of the minority Democrats in the House in 1900 and served as majority leader and chairman of the Ways and Means Committee from 1911 to 1915; he was also Senate minority leader from 1920

to 1923. Affable, patient, and hardworking, Underwood was a political insider who usually heeded the interests of his district, state, and section. He had three moments of glory: first, when he effectively ran the House as majority leader and helped enact Woodrow Wilson's "New Freedom" program; second, when he was a main contender for the 1912 Democratic presidential nomination, losing to Wilson; and, finally, when he ran a somewhat forlorn race for the 1924 presidential nomination, defying sentiment in Alabama and the South to attack prohibition and the Ku Klux Klan.

Underwood's chief significance, aside from his role in those events, lay in illustrating the conflict for an ambitious Southern politician between his sectional base and national aspirations. The beneficiary in 1912 of "stop Wilson" unreconstructed Southernism, he opposed prohibition and the Klan in 1924 not only through conviction but also in an attempt to attract Northern Democratic support. His candidacy failed largely because of lack of Southern support, and his economic conservatism as well as his prohibition and Klan stands so alienated his Alabama constituency that he would almost certainly have met defeat if he had sought reelection to the Senate in 1926.

This biography fits the man. Based on exhaustive research, it is detailed and makes no excessive claims for its subject. The concluding chapter, which recapitulates the book and competently assesses Underwood's character and career, will suffice for most readers, while those interested in particular events can gather further information from individual chapters.

JOHN MILTON COOPER, JR.
University of Wisconsin,
Madison

DAVID B. DANBOM. *The Resisted Revolution: Urban America and the Industrialization of Agriculture, 1900-1930*. Ames: Iowa State University Press. 1979. Pp. ix, 195. \$10.95.

This volume is partly a summary of the social and economic conditions of American rural life in the early twentieth century, partly an analysis of the efforts of urban critics of rural America to transform American farms into more productive and efficient units, and partly an assessment of the adversary relationship that developed between the traditionalist farmers and the city-based reformers. Its principal focus is on the ideas and programs emanating from the Country Life Movement, a nebulous coalition of nostalgic "urban agrarians," social scientists contemptuous of the backward rural folk, and a diverse assortment of businessmen, clergy, and agricultural scientists.

The participants in the Country Life Movement

regarded rural America as socially and economically static, insufficiently productive, and institutionally underdeveloped. In order to modernize this laggard sector of the nation they preached the gospel of urban-industrial efficiency to the farmers. While their specifications were disconcertingly vague, the thrust of their efforts was aimed at the reformation of three rural institutions—the school, the church, and government. They advocated school consolidation, the introduction of practical subjects (including nature study!) into the curriculum, the consolidation of rural churches, and the enlargement of rural government functions. Presumably the personnel who staffed these reformed institutions would imbue the farmers with the spirit of urban-industrial efficiency. David B. Danbom effectively summarized the essence of the movement in one sentence: "The Country Life Movement, stripped of all pretensions, was nothing less than the demand of an ascendant urban-industrial America, backed by an increasingly activist State, for an organized and efficient agriculture that would supplement it socially and economically" (p. 74). In short, the urbanites wanted a guaranteed supply of cheap food.

Rural Americans resisted the urban meddlers. The Country Lifers achieved little. Their major success, the institutionalization of the county agent system resulting from the Smith-Lever Act of 1914, was at best marginal. In the end, however, the farmers did modernize. But they did so because of World War I with its unprecedented demand for foodstuffs and because of the automobile with its destructive effect on rural parochialism. Agrarian self-interest, not the preaching of outsiders, produced the modern farm plant, and, eventually, agribusiness.

Danbom has written a fine book. It is sometimes repetitious, and perhaps the reality of rural-urban conflict is overstated. These, however, are minor flaws. It is well researched, cogently argued, and very handsomely produced.

KAREL D. BICHA
Marquette University

CARL W. CONDIT. *The Port of New York: A History of the Rail and Terminal System from the Beginnings to Pennsylvania Station*. Chicago: University of Chicago Press. 1980. Pp. xvii, 456. \$29.95.

Carl W. Condit, who has written extensively on urban architecture and the planning and building of American cities, asserts that despite the flourishing of urban history in recent years, there has been limited consideration of urban technology, a vital dimension of the history of cities. (The appearance of this study and several others in the last year or so may indicate that this is no longer the case.) Condit

argues that technology is also a relatively unexplored area of railroad history. His work supplies a comprehensive history of New York City's metropolitan railroad system—that is, of its standard railroads, with street railways and rapid transit excluded—from the beginnings to the building and operation of the central terminal at Pennsylvania Station.

Condit is exhaustive on all aspects of technology—construction, power, operation, and engineering. His extremely detailed, valuable chapter on railway electrification actually reprints an earlier, published, pioneering monograph. Condit's narrative, which probably presents scientific and engineering information in as clear a fashion as possible for the layman, is supplemented by extensive technical information in detailed notes and tables. He deliberately avoids corporate history or other aspects of the formal business history of railroad companies. But he goes far beyond narrow technological history in a second major part of his book—an examination of the relationship of railroad technology to the growth of New York City and American "urbanism."

He advances the thesis that the creativity and vitality of New York is related to "congestion with movement," to "urban circulation," and that the metropolitan railroad system therefore shaped the character of the city. He uses such writers as Edith Wharton and William Dean Howells to demonstrate the relationship of railroad technology to urban culture. A document, which occupies four pages of Condit's text, from W. S. Richardson, an architect who worked on Pennsylvania Station, provides a noteworthy statement on the relationship of the railroad to American civic design, and its inclusion provides a good example of Condit's sensitivity to humanistic considerations.

Condit argues that Pennsylvania Station was not only a technological triumph but remains the "greatest single work of building art so far undertaken in the United States . . ." Its demolition, he continues, "was perhaps the country's supreme act of urban vandalism" (p. 259). Condit's work is a definitive technical monograph that successfully relates a narrow subject to broader considerations of American urban history.

CHARLES N. GLAAB
University of Toledo

E. KIMBARD MACCOLL. *The Growth of a City: Power and Politics in Portland, Oregon, 1915 to 1950*. Portland: Georgian Press. 1979. Pp. xiii, 717. \$17.95.

This is the second of a proposed three-volume history of Portland. The focus of the present volume is on the development of a decentralized metropolitan region during the first half of the twentieth century.

The study is arranged topically, so that scholars who are studying other cities and wish to compare them with Portland on particular subjects may easily do so. The topics that are treated in most detail are those dealing with economic and political relationships. The volume contains extensive and rich information on banking, transportation, land use, and utilities. On a number of topics, E. Kimbard MacColl compares Portland with a number of other cities, and it is his comparative perspective that helps to make the chapters on utilities and transportation among the best in the study. In general, the volume is a gold mine of information.

While this is a useful study, MacColl has by no means exhausted the study of Portland during these years. There is some discussion of ethnic cleavage in the city, but we will still need a detailed and systematic study of Portland's social structure.

Despite the vast information on the economic and political systems of Portland, the study is somewhat weak on analysis and explanation. MacColl writes primarily descriptive history, and those who prefer this type of history will be more appreciative of the volume than those who attempt to integrate history with social science. Those who prefer the latter type of history will have good reason to be disappointed over the fact that a book with so much information on economic and political elites did not employ the methodologies of Robert Dahl in his study of New Haven, Connecticut, or Carl Harris in his study of Birmingham, Alabama. Because the methodology and theoretical framework of the study are somewhat diffuse, it is difficult to relate this study to other scholarship that has attempted to classify or to develop typologies of cities. Moreover, this volume will disappoint those scholars who are interested in a systematic understanding of how changes in economic structure, social structure, political structure, political culture, and public policy are interrelated.

Even so, MacColl has made a useful contribution to scholarship within the framework of his methodology. Those who would like a different type of analysis must still write a history of Portland during these years. But they will find much of the information that they will need in this volume and will be very much indebted to MacColl for his study.

J. ROGERS HOLLINGSWORTH
University of Wisconsin,
Madison

JOAN SHELLEY RUBIN. *Constance Rourke and American Culture*. Chapel Hill: University of North Carolina Press. 1980. Pp. xv, 244. \$16.00.

The pioneer cultural historian Constance Rourke was motivated by the need to discover a native tradition in the folk and popular arts. Rourke is best

known for her classic study, *American Humor* (1931), in which she examined the comic folklore of the backwoodsman, the Yankee, and the minstrels. During the 1920s and 1930s she also wrote popular biographies of Davy Crockett, Lotta Crabtree, John James Audubon, and Charles Sheeler. She also published essays on such diverse subjects as P. T. Barnum, Horace Greeley, Paul Bunyan, the Shakers, and vaudeville. Her innovative work uncovered new subject matter for American Studies scholars who are currently engaged in the writing and teaching of the folk and popular arts, and who, like Rourke, are influenced by cultural anthropology.

Thus Joan Shelley Rubin's book is a timely and important study. Her work is primarily a perceptive analysis of Rourke's writings. Only one chapter is devoted to Rourke's personal life. The other chapters are organized around the themes of tradition, culture, myth, style, and criticism. This format allows the author to scrutinize Rourke's work and discuss problems pertinent to American Studies methodology, chiefly the meaning of the term "culture" and the concept of myth. Partly through the use of Rourke's privately held papers, the author discloses valuable information about various individuals who influenced Rourke, including her mother, Frederick Jackson Turner, Johann Herder, Ruth Benedict, Jane Ellen Harrison, and Van Wyck Brooks.

The book has some minor shortcomings mainly because the author has selected a thematic organization rather than a chronological account. Rubin tends to underestimate the important relationship of Rourke's writing to the cultural history of the 1920s and 1930s, especially how her interests pertain to the vogues of regionalism and the folk arts during the Great Depression. Rubin believes that Rourke's writing did not change much over the years. But during her long writing career (1915-41), the quality of her work did vary. The curious fact that she wrote both mediocre books and groundbreaking studies is left largely unexplained. Rubin occasionally overlooks the weaknesses in Rourke's writing and even defends her for perpetuating myths about Davy Crockett and the frontier. Overall, however, these reservations are overshadowed by Rubin's interesting insights concerning Rourke and engaging discussion of major issues in American culture studies.

ARTHUR FRANK WERTHEIM
Arroyo Grande,
California

MATTHEW HALE, JR. *Human Science and Social Order: Hugo Münsterberg and the Origins of Applied Psychology*. Philadelphia: Temple University Press. 1980. Pp. xii, 239. \$17.50.

ROSCOE C. HINKLE. *Founding Theory of American Sociology, 1881-1915*. (International Library of Sociology.) Boston: Routledge and Kegan Paul. 1980. Pp. xiv, 376. \$35.00.

Roscoe C. Hinkle, a sociologist, analyzes the theory of the founders of American sociology with several aims in mind: among them, to discover what ideas were dominant in the period 1881-1915 and to establish continuities and discontinuities with later periods. The founding fathers espoused an evolutionary naturalism based on Darwinian views of organic evolution. Within this framework there were social evolutionists (Giddings, Keller, Ward) and social processualists (Cooley, Ross, Small, Sumner). The former had the concept of a fixed succession of stages; the latter (Sumner excepted), because of their idealist predilections, favored a theory of change that allowed more scope for human freedom.

Despite different theoretical orientations, the work of the early theorists was based on three major ideas: idealism, nominalism, and voluntarism. Idealism involves the notion that social phenomena derive substantially from consciousness. Only Sumner and Keller, with their materialist assumptions, deviated from this view. Social nominalism refers to the idea that the group has no independent reality, that its reality stems from the choices of purposive individuals. Voluntarism insists that social processes involve individual volition. Again, Sumner and Keller represented a minority tradition. Toward the end of the book, Hinkle suggests that these three assumptions characterize the theory of all later periods: the work of the Chicago School (1918-30); social action theory (1930-45); and structural-functionalism (1945-65). At this point, the author's presentist orientation surfaces. Like many practitioners who write disciplinary histories of their field, Hinkle treats the early figures in terms of present problems. In the current "crisis" of sociology, with structural-functionalism on the wane, many competing theories vie for influence. Hinkle uses the founders and their successors to confer legitimacy on some theories and withhold it from others. On the one hand, the materialist premises of exchange theory and the determinist leanings of neo-Marxism are clearly incompatible with the persisting assumptions of American sociological theory. On the other hand, phenomenology and symbolic interactionism, with their focus on consciousness and choice, are clearly heirs to the dominant tradition. Not only does this academic laying-on of hands occur. There is also an agenda for the future: students of theory should analyze the periods after 1915 with a view to demonstrating the persistence of idealism, nominalism, and voluntarism; and sociologists should

apply these three basic assumptions to the development of general theory.

Although it bears the marks of disciplinary history, this study differs from many of its kind in giving some attention to the historical context of ideas. Among other things, a connection is made between the values of progress and reform in nineteenth-century America and the founders' vision of sociology in the service of these ends. But the historical discussion is abstract and schematic.

Intellectual biographies can also fall within the tradition of disciplinary history, but Matthew Hale, Jr., deals with the ideas of the psychologist, Hugo Münsterberg, as a historian, not as a psychologist. Hale is not interested in Münsterberg's relationship to the founding fathers of psychology or to the current state of the field. Furthermore, he departs from the sociological approach to intellectual history that is present in much recent work on the history of the social sciences. Rather than use Münsterberg to illustrate the development of an academic community, Hale sets out to understand his ideas, to discover their political implications, and to relate them to the social thought of the early twentieth century. His indifference to the contemporary relevance of Münsterberg's views separates this study from Hinkle's. So does his use of historical context.

As a philosopher and psychologist during the "golden age" of Harvard philosophy, Münsterberg was both an idealist and a materialist. He employed neo-Kantian philosophy to uphold the realm of values, purpose, and consciousness. But this realm was radically separated from the practical world of human affairs, where a materialist psychology explained human behavior. This epistemological dualism had important consequences for Münsterberg's applied psychology. Developed and administered by experts, applied psychology bypassed consciousness and manipulated behavior in the interests of social efficiency and order. In industrial psychology and other areas, the goal was to strengthen the social ties that were frayed by modern life. And Münsterberg's notion of an organic society where individuals served the whole was the bridge between an older, German ideal that he brought with him to America and the Progressives' search for order.

Whereas Hinkle's discussion of the historical context of ideas is perfunctory and adds little to an understanding of those ideas, Hale makes the context crucial to such understanding. For example, to make sense of Münsterberg's blend of idealism and materialism, one must see it in the light of German academic life and thought (he came from Freiburg to Harvard at the age of thirty-three).

Context is not simply a background against which ideas are analyzed, as is true in Hinkle's case, but a tool for grasping the meaning of ideas. In

short, the sociologist's and the historian's approaches to the history of the social sciences continue to be quite different, and these two studies reflect those differences.

JEAN B. QUANDT

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ARTHUR G. POWELL. *The Uncertain Profession: Harvard and the Search for Educational Authority*. Cambridge: Harvard University Press. 1980. Pp. viii, 341. \$18.50.

A force against privilege, public education is the primary instrument for a common man's democracy. This idea, enthusiastically expressed by Elwood Cubberley, John Dewey, and George Counts, has not often been heard in recent years. Sociologists, economists, and social historians have turned the conventional wisdom on its head. The boundaries of the history of education have been stretched to fit such analytic models as bureaucracy, institutionalization, modernization, and professionalization.

Revisionists have centered attention on the power structure of educational organization: superintendents, experts, managers, schoolboards, finances, politics, and the status of social groups, especially privileged ones. Of little importance have been processes intrinsic to the activity of education: the classroom, the school teacher, professional preparation. Seeking to establish broad social relationships, the revisionists have highlighted the elitist control of America's system of education, a system of institutionalized inequalities functioning in its fullest ramifications as an integral part of a larger society with its built-in racism, sexism, and bias against ethnic and working classes. The explanatory models of the revisionists have tended to take for granted a functionalism that creates an impression of coherence and continuity in the educational system. Descriptions of ambiguity, discontinuity, and uncertainty deep within the historical experience have been missing or at least slighted in the historiography.

In *The Uncertain Profession*, Arthur G. Powell succeeds in describing confusion, not hidden agendas; irresolution, not conspiracy; small minds, not smart ones; unordered realities, not controlled ones. This book deserves attention. Powell presents a chronological narrative of the policies governing the study of education at Harvard, from Paul Hanus to Francis Keppel, from Eliot to Pusey. Traditional in approach, one might say old-fashioned, the perspective derives from the leadership of the institution. Drawing upon the rhetoric and reports of the insiders, Powell writes lucidly but gives more detail

than analysis. The story at times becomes heavy and the substance insipid. Nevertheless, Powell effectively portrays a moribund, inferior graduate school of education (Lowell once likened it to a "kitten that ought to be drowned") seeking legitimacy for its activity. The quest never succeeded: the faculty could agree neither on a body of knowledge nor on the problems that required solving; ephemeral circumstances pushed aside any consistent definition of professional purpose; and someone else usually did the job better.

A deep cause of professional self-doubt—here one wishes Powell was more analytic, concise, and imaginative—was that the academic leadership in the profession looked with disdain upon the teacher's task in the classroom, especially in the primary grades. Too often academic educators assumed that a mature and able teacher would be fed up with kids by the age of thirty-five. At Harvard, the sphere of professional training narrowed to serve male, administrative types, mid-careerists seeking credentials, and future academics, all ambitious for roles of leadership. The core of education was made peripheral as uncertainties inherent in growth, learning, teaching, guidance, and schooling were seldom confronted. Concepts of professional preparation were either too specific or too vague, often trivial, and sometimes absurd, undermining confidence.

BURTON J. BLEDSSTEIN
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Chicago Circle*

PAUL AVRICH. *The Modern School Movement: Anarchism and Education in the United States*. Princeton: Princeton University Press. 1980. Pp. xiii, 447. Cloth \$30.00, paper \$12.50.

Paul Avrich's new book is an important contribution to the understanding of the development of political and social thought in the United States in the twentieth century and the history of education. It traces the modern school movement from its beginnings in Spain in the early part of the twentieth century through its expansion in the United States between approximately 1910 and 1930 and its eventual demise after World War II.

The central figure in the founding of the modern school movement was Spanish anarchist Francisco Ferrer who established the Escuela Moderna of Barcelona in 1901 with the belief that the libertarian education of children was an important key to the creation of a nonauthoritarian society. Ferrer's ideas spread to the United States after his school was closed in 1906, and he was executed in 1909 by the Spanish government. His execution made him a

martyr in radical circles around the world. In the United States, important anarchists and radicals like Emma Goldman and Jack London condemned the execution, and, under the leadership of Goldman, Alexander Berkman, and other anarchists, the modern school movement was launched in 1910 with the organization of the Ferrer Association. In 1911 the Ferrer School of New York was established, and in 1915 the important radical community and modern school was established in Stelton, New Jersey. The Stelton Modern School lasted until 1953, and the Modern School Association of North America continued until the late 1950s.

The importance of this book for an understanding of the cultural and intellectual development of the period is in its detailing of the prominent artists and intellectuals who were associated with the modern school movement. Covers of issues of the *Modern School* magazine were done by Man Ray and Rockwell Kent. Will Durant taught at the Modern School and eventually entered a lifelong marriage with one of his students. Margaret Sanger, Elizabeth Gurley Flynn, Isadora Duncan, and Emma Goldman used the Ferrer Association as a platform for the advancement of women's rights. Avrich chronicles an almost endless stream of famous persons and radical movements that wove their way through the life of the modern school movement.

The importance of this book for the history of education is in its detailing of an education movement that specifically rejected public schooling as an instrument of corporate capitalism. Traditional histories of education often tell the story of the triumph of public schooling in the United States without mentioning that most radicals viewed it as a conservative victory designed to strengthen authoritarian government and the power of corporate capitalism.

Avrich's book is an important beginning in the study of the school issue in twentieth-century radical thought. The school is one of the most important institutions of the twentieth century and is reflective of attitudes about the nature of human beings and the organization of society. Since the time of Emma Goldman, the school issue has been an important concern to American radicals. This traditional concern has been reflected in recent times in the writings of Paul Goodman in the 1950s and 1960s and the radical school rebellion of the 1960s and early 1970s. Certainly future historians will have to give a great deal of space to the school issue when they write the history of the last two decades. In this regard, Avrich's book paves the way for a new beginning in the study of the role of the school in the development of American intellectual and social thought in the twentieth century.

JOEL SPRING
University of Cincinnati

LOIS SCHARF. *To Work and to Wed: Female Employment, Feminism, and the Great Depression*. (Contributions in Women's Studies, number 15.) Westport, Conn.: Greenwood Press. 1980. Pp. xiii, 240. \$18.95.

To Work and To Wed explores the impact of the Great Depression on female employment and feminism. While Lois Scharf does not dramatically alter the picture that appears in briefer treatments of the subject, such as William Chafe's in *The American Woman, 1920-1970*, she draws skillfully on primary source material to emphasize the main lines of that picture and to fill in the details.

The story Scharf tells is a discouraging one. If the depression was a disaster for men, it was an even greater one for women. Not only were female unemployment rates significantly higher than those of men (pp. 119-20), but their whole position in the labor force was threatened, since most Americans questioned the right of women to hold jobs when vast numbers of men were unemployed. Particular hostility was directed against married women workers on the frequently incorrect assumption that their husbands could support them. The results were predictable: women lost the modest foothold they had gained in the professions (a loss that was not made up with the return of prosperity in the 1940s); faced novel competition from men in fields they had long dominated; encountered campaigns to fire them from jobs they already held; and retreated steadily into lower-status and lower-paying jobs. One of the most interesting chapters of the book proves conclusively that New Deal programs, primarily concerned about the male head of household, consciously discriminated against women. Eleanor Roosevelt was the only important member of the administration to defend the female worker.

Scharf maintains that it was the massive reaction against women workers in the 1930s that finally destroyed American feminism. Those who had defended in principle the right of women to work and to seek fulfillment in careers lost whatever public influence they had ever had. An explicit contrast between the 1920s and 1930s underlies Scharf's whole discussion of this subject. She claims, incorrectly in my opinion, that feminism survived the passage of the nineteenth amendment but took a new economic form, which she calls "career and marriage feminism" (p. 34). The ideas and programs she includes under this rubric involve such compromises with traditional definitions of the family and the woman's role as wife and mother that the term feminist as she uses it becomes almost meaningless. William Chafe's interpretation of the 1920s as a period that saw a progressive decline of American feminism is far more convincing.

Despite this misinterpretation of a particular point, *To Work and to Wed* is a valuable addition to

our knowledge about women and work in twentieth-century America and is well worth reading.

BARBARA J. HARRIS
Vassar College

ROGER KEERAN. *The Communist Party and the Auto Workers Unions*. Bloomington: Indiana University Press. 1980. Pp. x, 340. \$22.50.

This is a revised version of Roger Keeran's 1974 doctoral dissertation, a history of Communist input in auto unionism over a period of thirty years. The Communists initiated activities in the early twenties when an industrial union in this open shop empire was only the dream of zealots. They shifted their energies to mobilizing unemployed in the desperate depression days. When the first spontaneous strikes erupted with the promulgation of the New Deal, and the AFL set up federal locals, party militants were in the forefront. Once workers moved massively under CIO aegis to form a union, it was inevitable that these by-then battle-scarred veterans would emerge to prominence in both national and local leaderships. Tiny in numbers, they could exert influence out of all proportion to their size because they had discipline, purposefulness, and central direction. Upon the transformation of the auto union from a crusade to an institution, Communists began losing ground until their decisive defeat and elimination by the victorious Reuther faction. Communist advantages had been dissipated by their members' display of unscrupulousness and robot-like responses to zigzagging party instructions, as well as the general hostility to a case-hardened Marxist formation responsive, to boot, to foreign manipulation.

Keeran has conscientiously checked sources and conducted his own interviews; but this ground has been worked over before, and he does not produce important new data. What is distinct about his book is that it is an apologia of monumental naiveté of the Communist party's schizophrenic career, an apologia that encompasses some of the party's factual misrepresentations, such as its invented contributions in the 1934 Toledo Auto-Lite strike or its alleged initial opposition to the sitdowners' vacating the plants in Flint in the 1937 GM strike. Anyone can make errors on details. Keeran's are part of a syndrome.

Moreover, since the party swung from ultra-left revolutionism to ultra-right social democratism and back again, and Keeran seeks to sympathetically explain the movements in all the moon's phases, he is left without consistent concepts on such matters as the character of the American working class or the role of Communist-type organizations in the antipathetic American environment. He criticizes the party in several asides, it is true, but these pas-

sages make painful reading because the criticisms are of the genre of closed-system official party "self-criticism."

The author is one of a number of young left-oriented labor historians who have demonstratively turned their backs on an older generation's cold-war anticommunism. These revisionist writers naturally take due cognizance of the yeoman labors of Communist activists in pioneering industrial unionism; by the same token, they are contemptuous of broadside attacks on Communists that can be made interchangeable with attacks on all manifestations of indigenous radicalism. Keeran, however, is unique among them in permitting his parochial sympathies to push him to positions that cannot withstand critical scrutiny, to demonstrating, in a variation of Gresham's law, how advocacy crowds out scholarship.

BERT COCHRAN
Columbia University

KARIN BECKER OHRN. *Dorothea Lange and the Documentary Tradition*. Baton Rouge: Louisiana State University Press. 1980. Pp. xvi, 277. \$27.50.

Among the more creative responses to the Great Depression of the 1930s was the documentary photography project undertaken by the Resettlement Administration and Farm Security Administration (1935-42). Supervised by Roy Stryker and conceived in the narrowest sense to publicize the operations and accomplishments of the New Deal agricultural programs, it transcended these limits and became the most ambitious photographic documentation ever undertaken of a way of life: American rural poverty, farm tenancy and sharecropping, migratory labor. In the process it lifted documentary photography out of the backwaters of photographic respectability, the occasional achievement of a Riis or Hine, and securely established the documentary vision in the mainstream of photography.

Dorothea Lange was one of a group of gifted photographers who were both the product of an era in American life and photography and the creators of a particular kind of documentary vision—humanist and reformist. This orientation was central to Lange, who viewed her work as a kind of photographic social science, documenting the hardships of the rural impoverished in words as well as photographs; she hoped that the photography would encourage the necessary political action to improve their circumstances. Although the concept of the documentary photo-essay was developed at the same time in the pages of *Life* and *Look* magazines, the emphasis was more on spot and news photojournalism than on social reform.

Karin Becker Ohn's biography of Dorothea

Lange overlaps previous publications such as those by William Stott, F. Jack Hurley, and Milton Meltzer. This volume is useful mainly for the account of Lange's career from 1939, when her ties with the Farm Security Administration ended, until her death in 1965. Plagued by bad health, she explored a new form of documentary expression after World War II (during which she produced her poignant photography of the Japanese internment): the new documentary vision focused on the intimate realm of family life.

ROY LUBOVE
University of Pittsburgh

D. CLAYTON BROWN. *Electricity for Rural America: The Fight for the REA*. (Contributions in Economics and Economic History, number 29.) Westport, Conn.: Greenwood Press. 1980. Pp. xvi, 178. \$22.50.

This book is a reminder that many of the big stories of the New Deal have still not been told. D. Clayton Brown offers the first scholarly study of the Rural Electrification Administration to appear in book form. The REA made low-interest loans to non-profit cooperatives established by farmers in local districts. In 1935, only one out of ten farms had electricity; in 1950, nine out of ten enjoyed comforting current. What electricity meant was nothing less than the lifting of rural families from a pre-industrial life.

Largely ignoring the conflict between the REA and private power interests, Brown focuses on the internal discord that plagued the REA. As early as the 1920s, proponents of public power could not agree on how far the government should go in serving the rural market. They were even more sharply divided over whether the power industry should participate as a partner. Senator George W. Norris, a leading proponent of public ownership of utilities, wanted the power companies excluded as a punishment for their failure to serve rural areas. But when the REA was created in 1935, the first administrator, Morris L. Cooke, went to the power industry with an offer of a joint program. If he had been successful, there would not have been any electric co-ops. After his overture failed, Cooke turned to cooperatives as a last resort.

Following roughly the same pattern, the Slattery-Ellis controversy during World War II centered on power generation. Harry Slattery spoke for the old-line progressives who wanted to work jointly with business within a limited conception of REA's mission. Turning his back on the power companies, Clyde Ellis represented a new generation anxious to expand REA into hydroelectric power. He called for "little TVA's," an echo of Norris's crusade for the Tennessee Valley Authority. In Congress, Sam

Rayburn used his influence to save hydroelectric dams in the Southwest from private control. But like Cooke, he accepted cooperation with business for the sake of efficiency, and "little TVA's" were too radical for the conservative postwar climate. In analyzing the REA's internecine conflicts, Brown sides with Cooke and Rayburn, but he denies that the REA promoted economic syndicalism.

Perhaps Brown's work will stimulate interest in the REA. The bitter struggle between the REA and private power companies offers a fruitful opportunity for further research. Brown has contributed an important monograph, judicious in its handling of conflicting viewpoints and virtuous in its brevity.

DONALD HOLLEY
University of Arkansas,
Monticello

intermingling of Army and Navy forces. One would also like to see a clearer concept of the Finance Corps' responsibilities for the Army's overgrown stepchild, the Army Air Force.

Despite these imbalances, this is a valuable and reasonably comprehensive work and it does have a thesis or "basic argument"—that the Finance Department made no adequate preparations for the exercise of its responsibilities in World War II and blundered along applying peacetime domestic procedures to overseas theaters where they worked poorly. The results of this blundering are set forth more elaborately in the author's earlier work, *Black Market Money*, but they do emerge in clear outline here.

ROBERT W. COAKLEY
Alexandria, Virginia

WALTER RUNDELL, JR. *Military Money: A Fiscal History of the U.S. Army Overseas in World War II*. College Station: Texas A&M University Press. 1980. Pp. xvii, 271. \$19.50.

In all the massive volumes of the officially sponsored U. S. Army in World War II series, there is no treatment of the administrative services, of which finance was one of the most important. Walter Rundell, Jr., attempts to fill part of this void in a volume on overseas operations of the U. S. Army Finance Corps based largely on research done on an abortive official volume as a Corps historian in the 1950s. Using a topical approach, he covers varied aspects of these operations—organization, fiscal diplomacy, budgeting and accounting, funding, payment of troops, civilians, and prisoners of war, currency exchange, and the life of troops serving in the Finance Corps. The problems and ramifications of paying troops and operating currency exchanges in wartime have seldom received attention from military historians, and this is a pioneer work. Its narrative and descriptive treatment preserves a record of an important activity in World War II.

Although the subject-matter breakdown is logical, treatment within chapters is episodic rather than systematic. There is a great deal of skipping around in time and place that could confuse the reader not well acquainted with theater organization in World War II (or for that matter with the acronyms by which some of these theaters were known). The result is sometimes uneven coverage of theaters and the peculiar problems that affected them. The chapter on administrative organization, for instance, includes only one of the Pacific theaters, omitting the most important one for the Army, General MacArthur's Southwest Pacific Area. And there is little insight into the problems that must have been generated in the Central Pacific by the

MICHAEL B. STOFF. *Oil, War, and American Security: The Search for a National Policy on Foreign Oil, 1941-1947*. New Haven: Yale University Press. 1980. Pp. xii, 249. \$15.00.

In assessing the place of the Middle East in American foreign relations during the 1940s, Michael B. Stoff writes, "War made the region prominent; oil made it vital" (p. 209). His purpose is to describe the first serious, but abortive, effort during World War II to effect a coherent national policy for foreign oil. He sees the wartime Anglo-American tensions over Middle Eastern oil as part of a transfer of power underway "all over the world and across a range of affairs" (p. 121). After establishing the links between petroleum and modern warfare, the author sketches the New Deal experience, which found oil companies "hospitable only to the species of government meddling that comes at the invitation of business and quickly falls under its control" (p. 7). Thus, during the 1930s, industry successfully enlisted state and federal cooperation for various devices to restrict production and stabilize prices.

Stoff illustrates his belief that individual personalities shape history with some sparkling sketches that illuminate conflicts within the wartime bureaucracy. He characterizes the "meticulous, irascible, suspicious, and incomparably honest" (p. 14) Harold L. Ickes in his capacity as Petroleum Coordinator for War as "the linchpin that held together disparate efforts to define national oil policy" (p. 12). Ickes's strategy for forcing the British to negotiate postwar petroleum arrangements on American terms led him into two clumsy, flawed efforts. First was the plan for government purchase through the Petroleum Reserves Corporation of all or part interest in American companies operating in Saudi Arabia and Kuwait. He then turned to a scheme for a government-constructed and -owned trans-

Arabian pipeline. Ickes's efforts not only alarmed the British, they also angered Secretary of State Cordell Hull, who was courting British support for negotiations that eventually produced the Anglo-American Oil Agreement of August 1944. Stoff laments the failure in the Senate of that agreement and its revision, and he explains how by the end of 1946 the private British and American companies worked out arrangements for their economic security. Government officials, in relaxing their continuous and consistent responsibility for petroleum policy, accepted the premise that private corporate interests and national interests coincided. The private arrangements provided advantages in the short run to the United States, Western Europe, and the Middle East, but the author concludes that "in the end the men who sought security promoted its collapse" (p. 215).

This soundly conceived and well-written book is grounded in extensive research; the author's use of British archival sources was especially productive. Our independent examination of much of the same material led the author and this reviewer to comparable conclusions; space limitations preclude discussion of minor points of difference. Stoff has made a solid contribution to the cluster of books recently published or in press on related themes. Together, the complementary volumes by Stoff, Bruce Kuniholm, Phillip Baram, Irvine Anderson, and Aaron D. Miller will aid those seeking a comprehensive understanding of United States foreign oil and Middle Eastern policies.

JOHN A. DENOVO
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GERALD J. BOBANGO. *The Romanian Orthodox Episcopate of America: The First Half Century, 1929-1979*. Jackson, Mich.: Romanian-American Heritage Center. 1979. Pp. x, 364.

Gerald J. Bobango, a young American specialist in the history of the Rumanians in the modern age and author of the recent *Emergence of the Romanian National State* (1979), has been enlisted to produce a historical survey of the Rumanian Orthodox Christian Church in the United States during the first fifty years of its episcopate, 1929-79. One could swiftly conclude that this work, published by the Romanian-American Heritage Center in Michigan, is designed to refute recent accusations against the church's bishop, Valerian D. Trifa, who has been cited as a former member of the fascist Rumanian Iron Guard during World War II and the perpetrator of war crimes against Rumanian Jews.

Unfortunately for the author and his publisher, this survey, treating much more than the current

Trifa episode, will immediately be seized upon by those who are intent on proving the innocence or culpability of Trifa. This book is not a source to cite in rendering judgment on Trifa, however.

For those who wish an indubitably favorable account of the church's experiences in the United States, this survey will probably remain for decades the authoritative account. Bobango first presents a brief survey of the first Rumanian settlements, then proceeds to the establishment of an organized church, and he then carries the progress of the church, together with its internecine squabbles, to the present time. For the secular historian the most captivating topics treat the relations between the American branch and its superiors in Rumania, a situation not unlike that experienced by the Russian Orthodox Church in North America and its relations with its original superior in Soviet Russia. Bobango does himself credit in diagnosing the delicate relationships between politics and religion, and he also presents the very secular task the church has faced in finding adequate financial resources to maintain its ministry in the United States.

One will clearly perceive the generous consent and support given the author by the current leaders of the Rumanian church, and for that reason the casual reader will conclude the survey is actually a paean to that church. Whatever its orientation, the extensive research undertaken by Bobango and his earlier visits to the Rumanian Socialist Republic to seek materials for his previous work indisputably suggest that the work under review is an honest and probably complete study. The book's publication during the emotionally charged case of Trifa's past and his future as a resident of the United States will detract from its acceptance as a scholarly work. Since so many of the Rumanian-Americans who fled their ancestral land to find justice and opportunity here should be accorded basic human rights, the same should also be granted to their bishop before he is adjudged guilty of illegal entry or mass murder of Rumanian Jews in 1941.

SHERMAN D. SPECTOR
Russell Sage College

JOHN WHITNEY EVANS. *The Newman Movement: Roman Catholics in American Higher Education, 1883-1971*. Notre Dame: University of Notre Dame Press. 1980. Pp. xvi, 248. \$14.95.

In 1907 two-thirds of the Catholics enrolled in institutions of higher education were attending a non-Catholic college or university. This trend remained consistent for most of the twentieth century and today has reached the point where four out of every five Catholics in college are students in a non-Catholic institution. Few Catholics get very excited

about this striking statistic today, but in the late nineteenth and early twentieth centuries it was a cause for concern. Out of this concern emerged the Newman Movement, an attempt "by Catholic students, campus officials, and clergymen to supply pastoral care and religious education in non-Catholic colleges and universities." This book examines the history of the Newman Movement from its beginning in 1883 at the University of Wisconsin to its coming of age in 1971 when approximately 1,450 chaplains were working with Catholic students on 1,200 campuses.

At first glance this book may appear to be very parochial and extremely limited in its focus—campus ministry among Roman Catholics—but to the credit of the author it is not. John Whitney Evans places this history within the context of religion in higher education as well as the history of American Catholicism. What emerges is a highly informative study that sheds a great deal of light upon the nature of conservative Catholicism in the first half of the twentieth century.

Not surprisingly the Catholic hierarchy looked upon the Newman Movement with suspicion. Any attempt to make the secular university appealing to Catholic students would, in the opinion of most Catholic leaders, only weaken the attraction of the Catholic college; to weaken the Catholic college would undermine the entire Catholic educational system and ultimately mean the collapse of the parochial school! This "educational domino theory controlled the thinking of most Catholic leaders" prior to World War II so that the Newman Movement "was almost done in," according to Evans, "not by the Catholic college, but by the parish school" (p. 83). Such thinking glaringly symbolized the parochialism of conservative Catholicism. Evans's analysis of this phase of the Newman Movement is the most rewarding section of the book. It is richly documented, well organized, and gracefully written.

The mainstreaming of American Catholics after World War II, the spirit of renewal ushered in by the Second Vatican Council, and the student movement of the 1960s reshaped the Newman Movement. In the past thirty years it has lost its third-class status and emerged as one of the most popular and creative ministries in the church. This phase of the movement is not analyzed by Evans with the same breadth and insight as the earlier period. Nor is the post-World War II section as well integrated with the broader issues of higher education and the development of American Catholicism as the earlier sections were. This is the only criticism I have of what is indeed an excellent study of an important issue in American educational and religious history.

JAY P. DOLAN
University of Notre Dame

EDWARD B. SHILS *et al.* *Industrial Peacemaker: George W. Taylor's Contribution to Collective Bargaining*. Philadelphia: University of Pennsylvania Press. 1979. Pp. xi, 244. \$22.50.

For more than a quarter of a century from 1946 to the mid-1970s the United States seemed to have constructed a successful and harmonious system of labor-management relations. Union leaders and corporation executives respected each other's needs and aims. Strikes resembled staged rituals more than bloody class conflicts. And union members benefited from rising real wages as great corporations accumulated ever more capital. This collection of essays represents a posthumous tribute to a leading architect of the post-World War II United States system of industrial relations, George W. Taylor.

Born in the Kensington section of Philadelphia in 1901, Taylor was educated at the University of Pennsylvania's Wharton School of Finance and Commerce, where he earned all three of his degrees, including the doctorate. From 1937 until his death in 1972, he served as a distinguished member of the faculty at the Wharton School. But Taylor was more famous for his public service and his practical achievements in the arena of industrial relations than for his academic career or scholarly contributions. His public service spanned the era from the New Deal to the Great Society and included work for the National Recovery Administration, the National War Labor Board, the Korean War Wage Stabilization Board, Eisenhower's Steel Strike Inquiry Board, and Kennedy's and Johnson's Advisory Committee on Labor Management Policy. Taylor also lent his name to the controversial New York state law governing collective bargaining rights for public employees.

The ten essays, all written by former students or colleagues of Taylor, largely explore the values and principles that governed his role in industrial relations. Taylor began with the assumption, in his own words, "that the objectives of organized labor and management are reconcilable, because their common interests are more compelling than the points of difference" (p. 57). Hence he believed that labor-capital disputes were always resolvable without violent conflict, that wise mediation and arbitration could avert strikes, and that government should *not* usually intervene in private collective bargaining. He also believed that union leaders and corporation managers shared a common interest in disciplining rank-and-file workers, most of whom had no conception of the proper bases for a harmonious industrial capitalist society. Taylor, moreover, refused even to concede public employees the right to strike. Although references and allusions to the concept of "democracy" filled Taylor's writings and speeches,

he clearly preferred a practice in which "professional experts" from business, labor, government, and the academy would devise and implement policies to serve the best interests of working people.

In revealing the principles and practices of post-World War II America's most eminent "industrial peacemaker," these essays are useful to students of modern United States history. But the essayists themselves exhibit slight sense of the meaning or process of history, fail to do justice to the English language, and frequently repeat each other. This volume belongs in a library, not an individual's office or home collection.

MELVYN DUBOFSKY
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JAMES CLAY THOMPSON. *Rolling Thunder: Understanding Policy and Program Failure*. Chapel Hill: University of North Carolina Press. 1980. Pp. xv, 199. Cloth \$14.00, paper \$6.50.

The failure of Operation Rolling Thunder to achieve in the Vietnam War the goals that the Pentagon set for it and the decision finally to call it off are landmarks in American military and political history. The confidence—indeed, the insouciance—with which the bombing of the North was undertaken memorializes the regnant assumptions of that era about this country's armed power and technological superiority. What was wrong about the theoretical underpinnings of the enterprise, what went awry in the execution of it, and how the defense establishment reacted to the frustration of defeat are the subject of this little book.

James Clay Thompson, a political scientist at the University of North Carolina at Greensboro, has addressed these three questions with the tools of his profession. The first two matters he deals with straightforwardly and unoriginally. He concludes: the recognition that Rolling Thunder had failed "led to the formation of an internal coalition that eventually became dominant within the command structure, and when it became dominant, policy changed" (p. 101).

Thompson's account of how the policymakers adapted to their increasing bafflement involves an extended examination of organization theory. The reader's mind is seized upon and then benumbed by the history and jargon of the subject. Further, Thompson so belabors the obvious—for the story is now an oft-told tale—that the reader must constantly remind himself that he is not acquiring a profundity he has hitherto missed on his own. Thus, every historian knows that in Vietnam the reports of aerial damage were often unreliable, sometimes excessively puffed up, and even occasionally created

out of whole cloth. In showy phrases Thompson tells us of the effect this phenomenon had on the authorities at home: "Lack of knowledge about what was being accomplished was . . . aggravated by indicators that did not present an accurate picture of Rolling Thunder operations. . . . Program effectiveness can be measured, of course, only by comparison of intended objectives with actual results. When the indicators adopted tend to distort the actual results, then the feedback mechanism of the organization breaks down" (p. 87). In the morass the interesting questions that Rolling Thunder posed are quickly lost. Thompson concludes in a brief epilogue: "[t]he strategic lesson—that the use of conventional bombing against a non-industrial country organized to fight and win a revolutionary war will fail—appears to be unlearnable." If conflict comes once more "advocates will again try to seduce policymakers with the will-o'-the-wisp of quick victory." Historians, knowing that predicting is not their work, can only admire the certainty of social scientists.

HENRY F. GRAFF
Columbia University

JOSEPH M. PETULLA. *American Environmentalism: Values, Tactics, Priorities*. (Environmental History Series, number 1.) College Station: Texas A&M University Press. 1980. Pp. xiv, 239. \$18.50.

Since the late sixties when a public concern developed about the state and future of the natural environment, numerous writers have tried to capitalize on the public interest. Historians, generally, have been slower to analyze the current situation in historical terms.

In order to understand the current situation, it is imperative to know the value system in which our attitudes and actions are rooted. The biocentric perspective was expanded by the Transcendentalists and their intellectual descendants. They recognized nature as an end in itself that should be protected against all contamination, even to the extent in recent years that natural objects should have legal standing in their own right.

The ecologic perspective recognizes the value of the natural ecosystems for man's use but is concerned about the actions of man that tend to upset the natural balance. Man can benefit from nature, but he must not be allowed to disrupt unnecessarily this self-regulating balance.

The third perspective is supported by those who advocate the wisest and most efficient use of nature over the longest period of time. This economic perspective believes that nature is to be used but that it is not to be wasted. Partly out of this perspective developed the concept of government involvement for

the protection and more efficient management of natural resources for the common good.

After establishing these three perspectives, Joseph M. Petulla examines each one in some detail, both historically and in the current context. Critical issues of the environment, such as the energy crisis in its environmental context, are discussed concisely.

This is the first volume in a new series, Environmental History Series, to be published by Texas A&M University Press. Petulla was a good choice to write the first volume, for he explains the historical perspective of environmentalism very well and sets the stage for future, more specific volumes. Historians and environmentalists dare not ignore this valuable work.

DONALD W. WHISENHUNT
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Tyler

PHILIP H. BURCH, JR. *Elites in American History*. Volume 3, *The New Deal to the Carter Administration*. New York: Holmes and Meier. 1980. Pp. xii, 544. Cloth \$19.50, paper \$14.50.

I have never read a more simple-minded book. It is a semantic and logical nightmare. "Elite," in this book, simply means some degree of corporate connection, not wealth, family standing, or achievement. Drawing only on secondary sources, such as *Who's Who* and business directories, Philip H. Burch, Jr., tries to determine the past corporate involvement of the highest ranking officials (cabinet officers, Supreme Court justices, and key ambassadors) in each administration from Franklin Roosevelt's to James Carter's. Any degree of involvement with large national corporations, from ownership to managerial position to honorary directorships on down to membership in any law firm with large corporate clients, makes one a member of the elite. Guilt may even come from association, as when one marries into a corporate family, has kinfolk or friends with corporate ties, or associates for too long with leading businessmen or their organizations. And guilt is the correct word, for Burch clearly loves only the pure in heart, the nonelite uncorrupted by corporate involvement. Despite all the problems of applying even this arbitrary definition, Burch still works out a score for each administration (the Carter Administration is over 65 percent elite; Roosevelt's was the all time winner with a low of 47 percent).

Such a mechanical counting of elites joins a naive, monistic theory of motivation. A corporate connection always means a commitment to certain policies, policies that Burch labels as "conservative" but rarely defines very clearly. For each administra-

tion, he tries to illustrate this sinister political role by a brief, superficial analysis of four or five major policy decisions. In his view, selfish economic interests totally control anyone infected with the corporate virus. On the other hand, nonelite officials seem immune to any selfish economic interests and choose policies out of a selfless devotion to the public good. This perspective gives a conspiratorial flavor to the whole book, particularly when Burch treats as major corporate fronts the Business Advisory Council, the Committee for Economic Development, the Trilateral Commission, and even the Council for Foreign Relations. However pure one has been (as a farmer, worker, or small businessman), any close contact with those organizations quickly makes one elite. James Carter caught the virus, not as a peanut merchant, but as a member of the Trilateral Commission. Dean Rusk, who never held a single corporate position, caught it from the prominent businessmen he met as president of the Rockefeller Fund. Even "liberal" (that is, good) behavior by an elite politician has to be hypocritical. Lyndon Johnson, fully corrupted by numerous corporate connections, necessarily supported his poverty program for political reasons. Carla Mills, Ford's secretary of housing and urban affairs, could not really be dedicated to the cause of the urban poor because she was a long-time member of a corporate law firm in Los Angeles.

PAUL K. CONKIN
Vanderbilt University

CANADA

DOUG OWRAM. *Promise of Eden: The Canadian Expansionist Movement and the Idea of the West, 1856-1900*. Buffalo, N.Y.: University of Toronto Press. 1980. Pp. x, 264. Cloth \$25.00, paper \$10.00.

The ways in which eastern Canadians viewed the Canadian West in the last half of the nineteenth century have long been of historical interest. Part of the story of how the positive impressions of the West contributed to its acquisition by Canada and its development by the metropolitan East have often been told in government documents, pamphlets, newspapers, periodical literature, and monographs. In this well-researched book Doug Owram carries the reader to another level by working the salient features of this story into a comprehensive analysis of Canadian expansionist thinking.

He takes a fresh look at the ideas behind the movement for westward expansion and discovers the usefulness and the shortcomings of the easterners' conceptions of the West through the years. The misleading view before 1850 that the Hudson's Bay Company territories were little more than cold

northern wilds discouraged Canadians from wanting to settle there. A more realistic image of the prairie region emerged in the fifties with the publication of Lorin Blodgett's *Climatology* and the coming of the Canadian and British scientific exploring expeditions. The climate and the fertility of much of the land, it was now thought, were such that the region could easily support a large farming population. This favorable opinion of the economic potential of the prairies was balanced by the belief that Palliser's Triangle, with its semi-arid climate, was of limited agricultural value. Thus, the mood that came to dominate the expansionist mind at the end of the fifties and during the next two decades was one of cautious optimism. The image of western agrarian wealth remained, however; and it was this image that helped to spur on the annexation of Rupert's Land. As the need for immigrants to develop the prairie region grew, the expansionists felt compelled in the early 1880s to bring more utopianism into their crusade. The initiative was now taken by the botanist John Macoun, who minimized the negative aspects of Palliser's Triangle and pictured the West as an agricultural Eden. There were some skeptics, but no one could foresee that in a few years, under the weight of the depression, the notion of Eden would be wholly inappropriate.

The author rightly sees in the expansionist movement an attempt to make the Canadian West over in the British Protestant image. It was to stand not in the tradition of the American frontier but in the tradition of English Canada. He notes the conspicuous role played by Ontario men in the movement. As a matter of fact, it was overwhelmingly Ontario, and so it seems odd to call it "the Canadian expansionist movement." Apart from this minor point, the book is a very good interpretation of what the expansionists thought and of what they managed to do.

HENRY C. KLASSEN
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GREGORY S. KEALEY. *Toronto Workers Respond to Industrial Capitalism, 1867-1892*. Buffalo, N.Y.: University of Toronto Press. 1980. Pp. xviii, 433. Cloth \$27.50, paper \$12.50.

Long before the appearance of this book—his first—Gregory S. Kealey has been the undisputed leader of what one of his colleagues has labeled "the second generation" of Canadian labor historians. Although some members of this group are in fact older than those in the so-called "first generation"—indeed, since serious labor history in this country is still in its first decade, the two "generations" exist side by side—this has not prevented some of its adherents from dismissing the studies of

those outside their circle as overly "traditional," too "institutional," and "often uninspired," nor from describing their authors as "naïve" and as "prisoners of their sources." The interests of this grouping are regional and specific rather than national and general; their writing is largely theoretical, concerned more with "culture" and class than with politics and unionism. Their gurus are, not surprisingly, Thompson, Hobsbawm, Gutman, and Montgomery. Unfortunately, unlike them, some of these young Canadian historians often allow their conclusions to outpace their data and their dogma to undermine their scholarship.

It is to Kealey's credit that this book far outdoes anything written by any of his associates. It combines the methodological strengths of both "generations" and successfully negotiates the pitfalls; indeed, it serves as a bridge between the two and indicates how artificial is the division. Like his mentor, Herbert Gutman, Kealey believes that the only legitimate way to recreate the experiences of the working class is through a community study. He could not, for his purposes, have chosen better than Toronto. Living in the industrial capital of Canada, Toronto's workers and their unions met and dealt with all the problems associated with the advent of industrial capitalism well before their brothers and sisters elsewhere in the country.

It is Kealey's contention that—at least in their places of work—Toronto workers were almost autonomous, that they exercised a surprising degree of control over their own lives. Indeed, he argues that some workers actually triumphed over the ravages of the industrial revolution and strengthened their authority in their shops and that in some trades the artisanal culture not only survived but flourished. More provocative—and less convincing—is Kealey's description of the roles of the Orange Order and the Knights of Labor in creating among Toronto workers a class consciousness.

The major flaw in this work—aside from Kealey's unwillingness, or perhaps his inability, to deal with the impact of ethnicity and religion on the "culture" of the working class—is its format. Although he writes well, Kealey has chosen to present his story as a series of case studies rather than as an integrated narrative. This makes his book less readable and his arguments more difficult to follow. Nevertheless, this is as fine a study as has been published on the Canadian labor movement and is certainly the best work yet on the nineteenth-century Canadian working class. So massive has been his research, so detailed has been his examination of sources, and so compelling are his conclusions that this study will long go seriously unchallenged.

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York University

LATIN AMERICA

KENNETH J. GRIEB. *Guatemalan Caudillo: The Regime of Jorge Ubico, Guatemala 1931-1944*. Athens: Ohio University Press. 1979. Pp. xvii, 384. \$16.00.

Jorge Ubico y Castañeda, Guatemala's dictator from 1931 to 1944, is probably best known to non-Central Americanists for his long tenure in office and his Indian labor policy that he called vagrancy laws. Now Kenneth J. Grieb, after a decade or more of study, has presented us with the first objective treatment of Ubico's official career. Like Ameringer's recent *Don Pepe*, this is not a biography (there is little family life, for example), but it is correctly labeled a study of the regime.

In this political analysis Grieb achieves definitiveness. Already known to all Central Americanists for his thorough scholarship, the author examines every aspect of Ubico's administration from agricultural production to water purification. The approach is topical, with heavy emphasis placed upon Ubico's economic problems and programs.

Central Americanists always lack secondary help, and that aphorism substantiates itself again here. Grieb attacked masses of documentation and, supported by a number of significant interviews, he has whipped the material into an orderly, sensible presentation. The author finds Ubico to be about as vicious as most of us have assumed, but he also portrays the caudillo as an able executive with a considerable degree of vision. Ubico's own models were Napoleon and Justo Rufino Barrios; Grieb sees a greater parallel to Porfirio Díaz with something of the Indian support enjoyed by Rafael Carrera. Whatever the style, Ubico gave personal attention to every detail of Guatemala's government, even frightening opponents with his knowledge of their intimate living habits.

Central American history often expresses such timelessness as to make it matter little during what decade a book is published. *Guatemalan Caudillo* follows a different pattern. My conversations with Grieb and the nature of his interviews make clear how long he has lived with his topic (nevertheless turning out much other work meanwhile). Not to criticize, I raise the matter only to wish that Grieb's epilogue could have more thoroughly addressed itself to the relationship of the Ubico years to the present chaos. Such effort would have additionally rewarded every reader.

Errors and omissions are few. Excellent maps and tables almost compensate for the lack of photographs—not even one of the caudillo himself. And I must insist that neither the middle class nor banana consumption took “quantum jumps” (pp. 34, 51) in the Ubico years. Also, the label “Colossus of the North” probably should not be used for both Mexico and the United States.

In another review I lately blessed the recent and splendid progress in Central American historiography. The next generation can consider itself fortunate to begin its work with a shelf full of books like this.

THOMAS L. KARNES
Arizona State University

GEORGE EDMUND CARL. *First Among Equals: Great Britain and Venezuela, 1810-1910*. (Dellpain Latin American Studies, number 5.) Ann Arbor, Mich.: University Microfilms International, for the Department of Geography, Syracuse University. 1980. Pp. xv, 171. \$17.75.

This history demonstrates that the lure of freedom of commerce with Great Britain provided some impetus for Venezuela's independence but that the latter's economic structure remained basically unchanged after the break with Spain. It details how British trade in the South American country during the nineteenth century surpassed that of the Spanish during the colonial era and documents how the course of Venezuelan economic growth was affected by dependence on Great Britain for manufactured goods. For example, the availability of low-priced English textiles and metalwares hindered the development of native industry because local entrepreneurs hesitated to compete with their British counterparts. Throughout the century unfavorable balances of trade existed between the nations, as Venezuela imported huge quantities of English goods, while Britain was uninterested in most of Venezuela's primary products.

George Edmund Carl provides insights into the origins of United States-British trade rivalry in Venezuela. He examines the evolution of the House of Boulton, one of the nation's most eminent commercial enterprises; explains how the *Caficultura* (coffee culture) of the nineteenth century, like the petroleum industry of the twentieth century, caused the economy to become so unbalanced that it was often necessary to import basic foodstuffs; and shows that by the nineteenth century Venezuela's leaders sought U.S. financing and technology and espoused the tenets of positivism—material advancement through order and progress. The author explores the relationship between Venezuela's nineteenth-century internal politics and external commerce more thoroughly than any historian writing in English, and he concludes that the nation's political situation, which oscillated between despotism and chaos, afforded little basis for economic development.

Carl presents sufficient evidence to make Venezuela an exception to D. C. M. Platt's theory that during the nineteenth century the wide gap be-

tween Britain's merchant and official classes prevented a close relationship between diplomacy and commerce and to disprove Platt's suggestion that the Foreign Office was composed primarily of disenchanted clerks who only wanted to maintain an open door for British goods in Venezuela. He notes that British diplomats encouraged legislation in Venezuela designed to promote closer commercial relations, thereby helping to erode the Latin American nation's economic autonomy. On the other hand, he states that foreign incursions into Venezuelan sovereignty were often overlooked by the Caracas government, which concentrated on more practical aspects of external trade. Carl supports the contention of Barbara and Stanley Stein that the Latin American nations remained dependent and colonial after independence because they had to buy and sell in order to survive. He also substantiates the generalizations about Venezuelan-British trade presented in the reviewer's *Diplomacy and Dependency*, which were predicated upon J. A. Hobson's theory of economic imperialism and assorted, recently postulated, dependency paradigms.

This well-written, but somewhat prosaic, monograph, based largely on archival records from Great Britain and Venezuela, is organized chronologically within a topical framework. The writer labored assiduously to locate materials and assemble them cohesively. The elimination of some insignificant facts would have given his work greater clarity. By addressing some incisive questions to his material or positing some theses, even as heuristic devices, he could have produced the stronger conclusions necessary to make a workmanlike book more definitive.

SHELDON B. LISS
University of Akron

JAMES LANG. *Portuguese Brazil: The King's Plantation*. (Studies in Social Discontinuity.) New York: Academic Press, 1979. Pp. xiv, 266. \$24.00.

James Lang has already established a reputation as a synthesizer of ability and insight on the basis of his earlier comparative history of Spanish and English America, *Conquest and Commerce* (1975). With the present volume he now adds Portuguese Brazil to this comparative perspective. Although specialists will find little new here, interested readers, especially those with a knowledge of other colonial regimes in the Americas but little of Brazil, will surely appreciate this briskly written synthesis of colonial Brazilian history based primarily on the recent monographic literature in English.

Mixing generalization with detail, Lang seeks to establish the distinctive elements of Portuguese colonization in Brazil. His finding is no surprise. Brazil

was an export economy so geared to European demand and royal control that it never made an adequate transition to self-sufficiency. By the late eighteenth century, however, Portugal was a third-rate power that was dependent on its American colony. It was willing to let the Brazilian elites have sway in the colony as long as the wealth of the colony continued to flow to Lisbon. The planters and merchants of Brazil had different ideas, and when Brazil became the dominant element in the colonial relationship, they moved toward independence. Lang makes a great deal of the seeming differences between the Spanish and Portuguese American colonies. He finds much of his explanation in the intense bureaucratization of the Spanish empire and the institutionalization of life in its colonies. This, plus the internal self-sufficiency of places like Peru and Mexico, explains much of the divergence between Brazil and Spanish America. One could point out, however, that while the Brazilian bureaucratic structure was surely smaller and less developed, it also served a much smaller population. The size and density of the Indian people encountered was also quite different. Perhaps it is better to compare Brazil with New Granada or Venezuela, not Mexico or Peru; when such comparisons are made, the remaining differences in Spanish and Portuguese methods and goals raise deeper questions than those answered here.

There are enough typographical and factual errors in this book to be distracting, but a more serious problem is Lang's heavy dependence on the existing literature in English for much of his analysis. The works of Alden, Boxer, Maxwell, Russell-Wood, and myself are so often cited that I had a certain sense of *déjà vu* in reading some chapters. The relatively few monographs in English on colonial Brazil have left unexplored a vast number of topics while concentrating on others. Thus, bureaucracy and trade take center stage in Lang's interpretation to a certain extent because these have been central themes in the recent monographic literature. The problem, however, is that other themes, such as demographic change or the internal organization of the economy, have remained less explored in these recent works and thus receive slight attention here. While there are close to two hundred citations of works by the five authors mentioned above, there is no use made of the respective work of Fernando Novais, Maria Luiza Marcilio, or J. A. Gonçalves de Mello. Foreigners have made important contributions to colonial Brazilian historiography, but there has simply been too much of value done in Brazil itself to be overlooked; furthermore, much of it has emphasized themes and positions quite different from those of the "Brazilianists." As a synthesis, then, Lang's book is limited by his selection of sources, but it does bring together

a great deal of information and may provide a long-needed text for college courses on Brazil.

STUART B. SCHWARTZ
University of Minnesota

TIMOTHY E. ANNA. *The Fall of the Royal Government in Peru*. Lincoln: University of Nebraska Press. 1980. Pp. xi, 291. \$19.50.

The uniqueness of the Peruvian experience in the achievement of nationhood is the theme of Timothy E. Anna's second monograph on the independence movements in nineteenth-century Spanish America. Admirably documented from archival sources in Spain and Peru and well written, it proposes a more realistic approach to an understanding of the indecisiveness of the Peruvian elite in the solution of the political crisis. Historians have been divided in assessing the determining factors in this "profoundly troubling" question. To the fervent nationalists, success was due to individuals inspired by love of country and hatred for Spain. To others, the winning of freedom was the work of foreigners, of San Martín, Bolívar, and Sucre.

In Anna's account of the revolution the key to realities is the economic impoverishment and unproductiveness of the region. By 1800 the Peruvian economy had greatly declined owing to the creation of the viceroyalty of the Rio de la Plata, which cut off markets for agricultural commodities and rich areas for exploitation. Other causes were the decrease in maritime trade with Panama, expansion of contraband, shortage of labor, and backwardness of the mining industry. Associated with this deplorable economic condition were also apprehension of social upheaval and monopoly of offices by Spaniards.

It is indeed plausible that the multiplicity of adverse factors precluded the formation of a well-organized group of patriots. In contrast to developments in other regions during the revolutionary era, ideology does not appear to have been a strong motivation. Peruvians might, however, challenge the bold assertion defining the elite mainly "as an over-privileged minority seeking to select from among alternative governmental systems the one that would allow them the greatest share of wealth, prestige, power, self-fulfillment, or whatever else they sought" (pp. 17-18). Confronted by the varying fortunes of the opposing forces, weighing the advantages of liberty or continuation of Spanish rule, the practical-minded dominant group could not agree on a single course of action. The outcome was, in the author's view, "freedom by coercion."

One of the innovative and commendable features of this study is the extensive presentation of the royalist side of the revolution. Viceroy Abascal and

Pezuela emerge as humane, dedicated officials in contrast to the popular caricatures of tyranny and corruption. This revisionist point of view, hitherto neglected by Hispanicists, merits greater attention.

Nevertheless, there are aspects of the Peruvian revolt requiring further clarification. Why was the animosity of creoles and peninsulars—the usual concomitant of this civil conflict—felt to a lesser degree? What is the evidence of native discontent or threat of uprising? These omissions should not detract from this solid, well-rounded investigation. Scholars will welcome it as an antidote to chauvinistic rhetoric and bias.

J. PRESTON MOORE
Louisiana State University

JACQUES A. BARBIER. *Reform and Politics in Bourbon Chile, 1755-1796*. (Cahiers d'Histoire, number 10.) Ottawa: University of Ottawa Press. 1980. Pp. xiv, 218. \$9.75.

This study of administrative and economic reform in colonial Chile during the latter half of the eighteenth century provides both a wealth of historical detail and gems of historical synthesis. For the reader-researcher generally concerned with the role in colonial society and politics of royal officials, landowners, merchants, and elite families, Jacques A. Barbier extracts from the primary sources the pettiness and the graft as well as the values, valor, and accomplishments of peninsular and *criollo* elites. For the reader concerned more with overall trends or with traditional historical issues, Barbier consistently supplies the cryptic bottom line: "the basic unit of political action in eighteenth century Chile was the clique"—based on family ties, *compadrazgo*, place of birth, common corporate membership, and business interests. Therefore, the "struggle between Creoles and Spaniards was far from being overwhelmingly significant" (pp. 75-76). "Financial reform . . . threatened local interests. So long as Chilean administration was corrupted by local interests, it was difficult to carry out any effective changes" (p. 77). "The Bourbon *visitas* were meant to nurse the empire back to fiscal health. In Chile the policy was counterproductive" (p. 127).

This book is researched carefully and acknowledges appropriately debts to Chilean and non-Chilean scholars—even as it relies essentially on archival and other primary sources. It self-consciously addresses administrative and political history, fiscal problems, and elite politics and maneuverings. It is not a social history of eighteenth-century Chile, nor does it make any attempt to inform the reader of how 90 percent or more of Chileans lived day by day from the mid-eighteenth century onward, though occasional glimpses are provided, for ex-

ample, in a description of a *corregidor*'s use of convicts or unlucky "vagrants" on his pet construction projects or in Barbier's conclusion regarding alternative trade policies at the end of the eighteenth century: "nor would protectionism have constituted an effective response, for a domestic market of less than half a million people, most of them poverty stricken *huasos*, hardly offered much inducement to investment" (p. 160).

What Barbier sets out to do in the study, he does exceedingly well—his research is an outstanding contribution to our knowledge of politics, administration, and economy in late-eighteenth-century Chile. The complex, overlapping, episodic, and consistently corrupt and petty character of Spanish imperial bureaucracy in Chile is clearly documented and skillfully—sometimes humorously—chronicled. The formation of alliances among different cliques of the older elite families, royal officials, merchants, and landowners dispels the stereotyped characterization of the *cabildo* as the base of *criollo* opposition to the otherwise peninsular-dominated royal bureaucracy. "The audiencia of Chile

was far from being merely an agency of the Crown. The *oidores* were predominantly creoles and had bought their offices" (p. 74). Likewise, the reader cannot help but be impressed with the resiliency with which *criollo* elites in Chile obstructed, subverted, weakened, or perverted efforts to strengthen royal control and to increase royal income from the Chilean colony.

Finally, and perhaps even more impressive in light of the history of later attempts at social and economic reforms in Chile in the nineteenth and twentieth centuries, is Barbier's finding that a major impact of the Bourbon reforms was to increase economic and social mobility for upper- and middle-strata families by expanding the state bureaucracy (p. 122). This finding is an important reminder that, in the Spanish imperial system, there could be no fine distinction between "public" and private enterprise, no business without politics, and little advancement of family position without appropriate connections to the political realm.

BRIAN LOVEMAN

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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

JAMES H. SOLTOW, editor. *Essays in Economic and Business History: Selected Papers from the Economic and Business Historical Society, 1976, 1977, and 1978.* (MSU Business Studies.) East Lansing: Michigan State University, Graduate School of Business Administration. 1979. Pp. x, 326. \$8.00.

DIETRICH G. BUSS, Henry Villard and Thomas Edison: Growth of Incandescent Lighting, 1878–1892. RONALD L. F. DAVIS, Southern Merchants and the Origins of Sharecropping: A Case Study. NOBUO KAWABE, "Made in Japan": The Changing Image, 1945–1975. J. MALCOLM WALKER, Organization and Management Theory in the Soviet Union: 1917–1950. DENA S. MARKOFF, The Entrepreneurship of Francis King Carey. RICHARD J. OESTREICHER, Changing Class Relations in Detroit: 1880–1900. NEAL HIGGINS, Two Early Pension Plans: The B&O and Pennsylvania Railroad Programs. WILLIAM D. CRIST, Public Sector Collective Bargaining: California's Public Universities and Colleges. GILBERT L. MATHIS, The Legal-Economic Process in Twentieth-Century America. THEODORE P. KOVALEFF, The Electrical Cases: Twenty Years Later. PHILIP R. SMITH, Consumer Sovereignty, Choice, and Strategy in the United States: 1870 to the Present. MILTON ESBITT, The New York City Banking Suspension of 1837. MILTON ESBITT, An Antebellum Attempt at National Bank Regulation: Deposit Banks. DENNIS O. FLYNN, Silver and the Spanish Empire. REINHOLD SCHUMANN, Coin Scarcity in Italy and Money Substitutes: 1973–1977. WILLIAM T. CARLISLE, Theories of the Great Depression: 1929–1933. LARRY HUFFORD, A Survey of Nordic Cooperation. LARRY HUFFORD, Swedish Socialist Debates of the 1920s. K. PETER HARDER, The American Economy in 1776: Some Perspectives. KENNETH WEIHER, Slavery and Southern Urbanization: A Reformulation of the Argument. SAUL ENGELBOURG, Energy and Industrialization: The Case of Southern New England. VIRGINIA LEE OWEN, Gothic Cathedral Building as Public Works. VIRGINIA LEE OWEN, Economic Influences on the French and American Impressionist Movement.

W. D. RUBENSTEIN, editor. *Wealth and the Wealthy in the Modern World.* New York: St. Martin's Press. 1980. Pp. 283. \$25.00.

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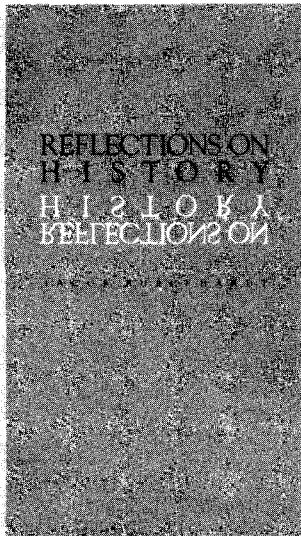


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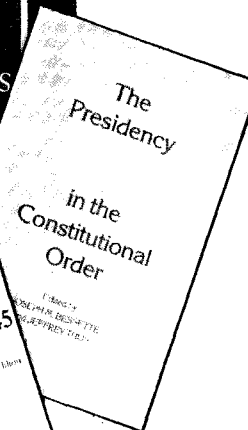
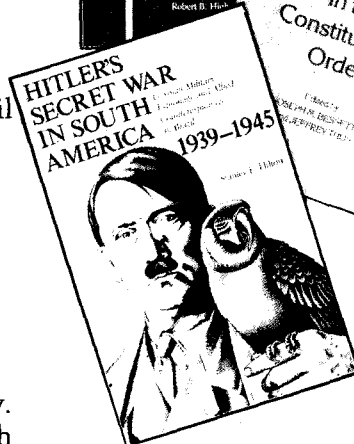
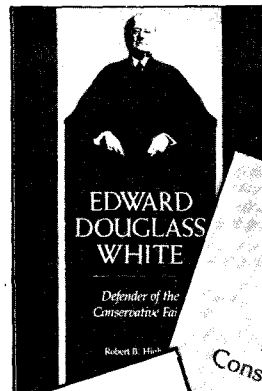
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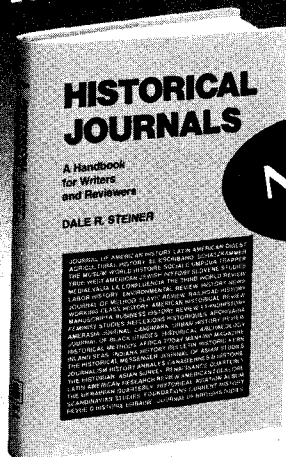
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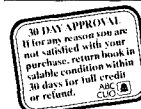
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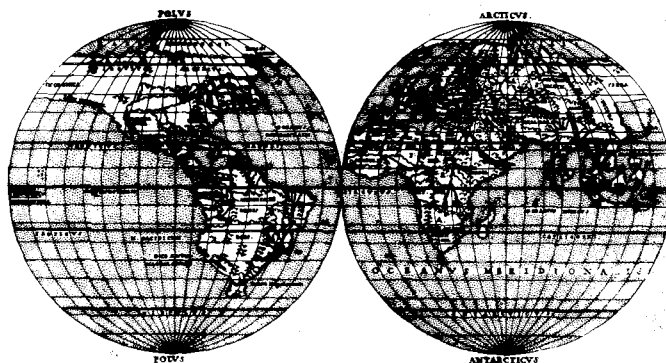
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